The Journal of
NERVOUS and
MENTAL DISEASE

VOL. 128, NO. 1
January, 1959
SERIAL NO. 915

PHARMACOLOGICAL SELECTIVITY MANIFESTED BY AGENTS ACTING UPON THE CORTICAL DENDRITIC SPIKE AND ITS SLOW AFTER-EFFECTS[1]

SIDNEY GOLDRING, M.D., JOHN S. METCALF, M.D., SHI H. HUANG, M.D.,
JOHN SHIELDS, M.D. AND JAMES L. O'LEARY, M.D.[2]

A recent theory ascribes the origin of alpha rhythm to the cortical dendrites, thus making important the accumulation of data concerning potentials arising from the dendritic plexuses as they are altered in pathophysiological states. For such study the action of depressants has special significance because of the diverse uses to which the depressant drugs are put clinically. For evaluation the anesthetics provide important comparative data, and the varied means of producing an hypoxic state provide another essential control.

As shown in studies by Adrian (1), Chang (2), and Clare and Bishop (3), a negative potential (or spike), assignable to the superficial dendritic plexuses of the cortex, can be activated from as far as 5 mm. from the tips of the surface stimulating electrodes. Each such spike response to a single stimulus is followed by a slower potential disturbance the origin of which is less well understood. In the cat this slow sequela ordinarily appears as a low voltage positive wave lasting 100–150 msec. (Figure 1). That positivity can be shown to submerge much of an even longer lasting (150–200 msec.) negative wave. The latter can be uncovered either by increasing the voltage of the single stimulus or by substituting for that stimulus serial shocks of equal intensity (Figure 1). In part, at least, the negative wave which follows a single stimulus can pass the condensors of a conventionally coupled amplifier and appear in the tracing in abbreviated form. Thus, that wave has been recognized previously and interpreted by Chang (2) to signify activity in intracortical neurones. By contrast, we have viewed the non-distorted form of the same wave as a continuation of the activity represented in the evoked response and capable of summation during repetitive stimulation. We have designated this after-negativity and the after-positivity described previously as after-effects, a usage broad enough to leave their origin to be decided by the evidence as it accumulates (4). Present evidence supports increasingly a difference in the origins of negative spike and of after-negativity, and we know too little as yet about after-positivity to hazard any guess as to its neuronal origin.

Recently we studied in the cat the result of deepening anesthesia upon the cortical dendritic spike and its after-effects (4). The changes we observed led to the present study and are summarized briefly here. A d.c. amplifier and non-polarizable electrodes were used, with a potentiometer introduced into one arm of the recording

---

[1] Aided by Grant B1517, United States Public Health Service, and in part by a grant from the Allen P. and Josephine B. Green Foundation.

[2] Divisions of Neurology and Neurosurgery, The Division of Anesthesiology and the Beaumont-May Institute of Neurology, Washington University School of Medicine, St. Louis. The authors wish to acknowledge their thanks to Dr. Thomas F. Hornbein, who participated in some of the experiments, and to Miss Carol Kloeris, who provided editorial assistance.

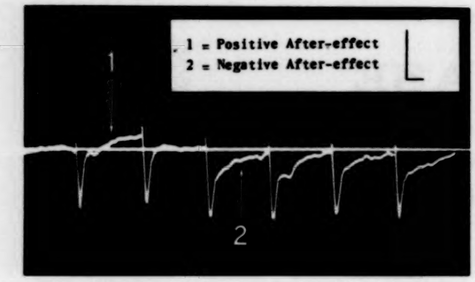

FIG. 1. Repetitively evoked (6/sec.) dendritic spikes and their after-effects in the cat. Note that a negative after-effect replaces the positive one by the time the third response is elicited. In this and all subsequent figures the straight horizontal line represents an arbitrary baseline against which all potential changes are read. Positive polarity is up. Vertical line of calibration symbol, 500 μV; horizontal line, 40 msec.

circuit to permit the equalization of the resting potential between the two recording arms (6). Terminologically, evoked changes having the time dimensions of the after-effects under study here are called d.c. or slow potential changes, and the steady state of activity which characterizes intervals between stimulation is designated SP (steady potential). SP itself may shift positively or negatively under different circumstances, as, for example, during deepening barbiturate anesthesia it shifts first positively and then negatively.

Prior to giving nembutal a singly evoked dendritic spike was invariably followed by a surface-positive after-effect. Using the same stimulus voltage but serial stimulation at 6 per sec. that positive was replaced by a negative after-effect as the stimulus series continued. With serial stimuli at 20–30 per sec. negative after-effect summation was obtained throughout, the change lasting 1–2 sec. after the end of a stimulus series. Following the iv. administration of 30–75 mgm. of nembutal the after-positivity following the singly evoked response disappeared, a negative after-effect replacing it. Also, with serial stimulation after this dosage the serially elicited dendritic spikes and their summed after-effect were affected differentially, spike amplitude being reduced while that of the summed after-effect both increased and became more prolonged. Using treble the dose of nembutal cited above serial stimuli beyond the first no longer elicited spikes, whereas the summed after-effect was further augmented and prolonged, then coming to outlast the stimulus period by 3–5 sec. We presumed the loss of spikes and the increase in amplitude and prolongation of after-negativity with deepening anesthesia to result from less effectual repolarization of neurones after activity, and to be a sign of depression. If correct, this hypothesis should apply as well to a variety of other agents. Thus in the present study we tested procaine, chlorpromazine, benadryl, nitrous oxide, and ether. Other systemic factors which could complicate interpretation were rigidly controlled by performing separate experiments in which excesses of carbon dioxide, anoxia and hypotension were studied.

METHODS

Cats weighing 1.5 to 2.0 kg. were prepared under ether anesthesia following which they were immobilized with Flaxedil (gallemine triethyliodide) and maintained upon artificial respiration. Recording was commenced 1–1½ hrs. after cessation of anesthesia. A clinical anesthesia machine (concentration of gas delivery controlled by a rotameter) was connected to the intake of the respirator and used to deliver the volatile anesthetics and other gases. Preceding the introduction of one of these gaseous agents the cat was respired with 100% oxygen during a control period. The other agents studied were given intravenously, and these animals were ventilated with room air during the control period. No significant differences were noted between animals respired with room air and with 100% oxygen. Intravenous injections were made through a polyethylene catheter threaded through the femoral vein into the cava. For monitoring blood pressure and pulse the femoral artery was cannulated with a Huber needle which was connected to the lead arm of a Sandborn electromanometer.

Calomel electrodes were used for recording, the critical electrode being placed upon the anterior suprasylvian gyrus and the reference one on the occipital bone. The stimulating electrodes were placed within 0.5 mm. of the critical calomel electrode and consisted of three 100 μ nichrome wires arranged in a triangle. Each wire was insulated to its tip and the interelectrode distance was 75 μ. The wires were led through a circuit devised by Landau (5) for shock artifact control. Two Grass stimulators were used, one to deliver the stimulus and the other to control the length of a period of serial stimulation. In all instances one sec. bursts of 6 and 20 per sec. stimuli were employed in addition to single shocks. Stimulus strength was 5 to 10 v. at .01 msec. duration. A d.c. amplifier, oscilloscope and camera were used for recording.

RESULTS

The straight horizontal line in all figures is the arbitrary baseline which we define at the start of an experiment by balancing the voltage difference between recording electrodes through the use of the variable potentiometer in the recording circuit. Shifts of the tracing above this line constitute a *positive* change, shifts below it a negative one. The polarity is referred, of course, to the electrode placed upon the cortical surface.

1. *Nembutal.* Our experiments with nembutal were first repeated, recording the blood pressure which was obtained when the records were taken. The results give more detail than previously. The initial effect of injecting nembutal was to change the SP across the cortex positively with respect to its state preceding the injection. With additional injection of nembutal, the SP continued to shift slowly positively. The slow potential changes resulting from stimulation were superimposed upon this shifting potential of the resting cortex.

Nembutal was injected slowly over a 1–2 min. period in equal 30 mgm. doses. A transient blood pressure drop might succeed each such injection, but a wait of several min. restored normotensive pressure. It was then that the records were taken. However, after injections amounting to 150–300 mgm. the pressure might remain low, hovering between 60–80/40–60.

After the injection of 30–60 mgm. a negative after-effect of a singly evoked dendritic spike was observed to replace the usual positive one. (Compare responses 1, 2, 3, uper tracing, Figure 2 A, with corresponding responses in Figure 2 B). The irregularity in appearance of negative

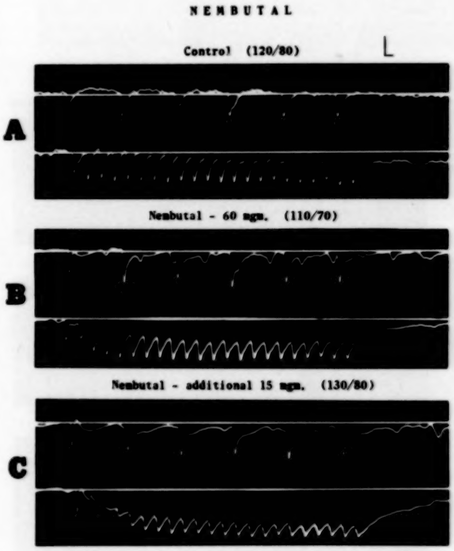

FIG. 2. Effect of nembutal in serially evoked dendritic spikes and their summed negative after-effect. In A, B and C stimulus frequencies are 5 and 20 per sec., upper and lower traces respectively. A, prior to nembutal injection. B, after iv. injection of 60 mgm. nembutal given in two injections of 30 mgm. each. C, after an additional 15 mgm. nembutal. In this and all subsequent figures, numbers to right of tracing refer to blood pressure. Vertical line of calibration symbol, 500 μV; horizontal line, 20 msec.

after-effects in Figure 2 B is due to spontaneous accentuation of the ECG rhythms by nembutal. With serial stimulation at 20 to 30 per sec. the successively evoked dendritic spikes progressively decreased in amplitude, the slow potential change resulting from summation of after-negativity increasing significantly (compare lower tracing, Figure 2 A, with that of Figure 2 B). An additional 15 mgm. of nembutal exaggerated the change just described (Figure 2 C). Indeed, with nembutal totaling 150–200 mgm. the spikes failed to be evoked beyond the first of a series of stimuli, the response to serial stimulation coming to consist only of the initial spike followed by an exaggerated summation of negative after-effect lasting through the stimulus period and significantly into the post-stimulatory trace. This change would be missed in tracings obtained with a condensor coupled amplifier.

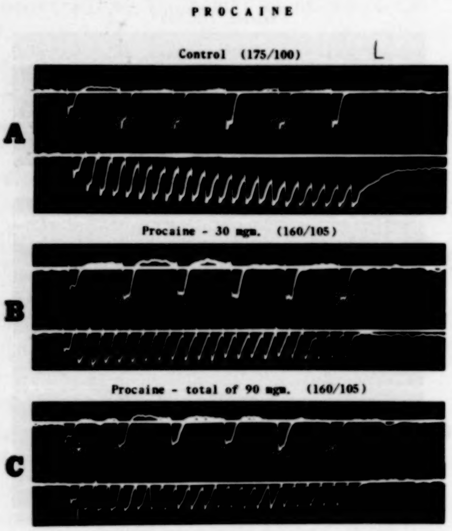

Fig. 3. Effect of procaine on serially evoked dendritic responses and their summed negative after-effect. In A, B and C stimulus frequencies are 5 and 20 per sec. for upper and lower traces respectively. A, before the injection of procaine. B, after iv. injection of 30 mgm. procaine. C, after a total of 90 mgm. In both B and C procaine was given in divided doses of 15 mgm. each; a 5 min. interval separated each injection. Vertical line of calibration symbol, 500 μV; horizontal line, 20 msec.

2. *Procaine.* When small amounts (15–30 mgm.) were injected slowly neither pulse rate nor blood pressure was affected. However, larger amounts (60 mgm.) injected rapidly could produce cardiac arrest and death. Only in the latter circumstances did procaine produce a significant SP change.

The effect of procaine on the stimulation record was the virtual opposite to that of nembutal and this observation is believed consistent with the known opposition between the clinical effects of these drugs. At 30 mgm. the usual summation of negative after-effect did not occur with 20 per sec. serial stimulation (compare the lower tracing of Figure 3 A with that of 3 B, and recall the exaggeration of the same after-effect by nembutal). This change was most marked immediately following an injection, partial recovery occurring later. By contrast the serially stimulated dendritic spikes (5 and 20 per sec.) were unaffected at this time (Figure 3 A and B). However, with the injection of additional procaine to a total of 60–100 mgm. the 6 and 20 per sec. evoked spikes diminished somewhat in amplitude (Figure 3 C). Still more procaine caused seizure activity to develop and reliable results were no longer obtained. The blood pressure of the animal from which the records of Figure 3 were obtained was elevated. However, this was not believed to influence the results because corresponding findings were obtained in normotensive animals.

3. *Chlorpromazine.* When given slowly and in small amounts (10 mgm., 1–2 min.) this agent produced only a transient drop in blood pressure and no significant SP changes. However, both the dendritic spike and the negative after-effect summation with serial stimulation at 20 per sec. were augmented, that change occurring with a single injection of 10 mgm. (compare Figure 4 B with 4 A) and becoming maximum

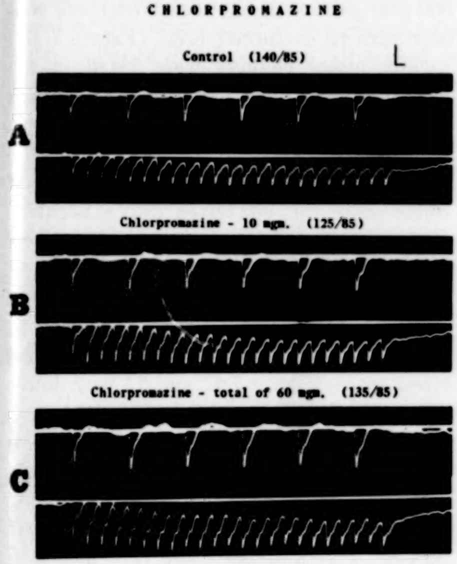

FIG. 4. Effect of chlorpromazine on serially evoked dendritic spikes and their summed negative after-effect. In A, B and C stimulus frequencies are 5 and 20 per sec. in upper and lower traces respectively. A, before chlorpromazine injection. B, after iv. injection of 10 mgm. C, after iv. injection of a total of 60 mgm. In both B and C chlorpromazine was delivered in divided doses of 5 mgm., at 5 min. interval separating each injection. Vertical line of calibration symbol, 500 μV; horizontal line, 20 msec.

after successive injections totaling 30–70 mgm. (Figure 4 C). Additional chlorpromazine brought out seizure activity, and at this point an experiment was stopped.

4. *Benadryl* (diphenyhydramine hydrochloride). Like chlorpromazine, this agent occasioned only transient drops in blood pressure and even these could be eliminated if the injections were given very slowly over a 2 min. period. Following 10 mgm. of benadryl the SP shifted positively by 0.5–0.75 mv, that shift developing over a 30 sec. to 1 min. period, continuing for a corresponding length of time, and requiring somewhat longer to return to the baseline (Figure 5 B). Such a post-injection SP shift could be elicited more than once in the same animal, recurring with each 10 mgm. injected. However, after several such injections (totaling 50–70 mgm.) seizure activity entered the tracing and the change could no longer be produced. Such seizure activity occurred in bursts, each accompanied by a negative slow potential change (Figure 5 D).

No change in the serially evoked dendritic spikes (6 or 20 per sec.) was noted

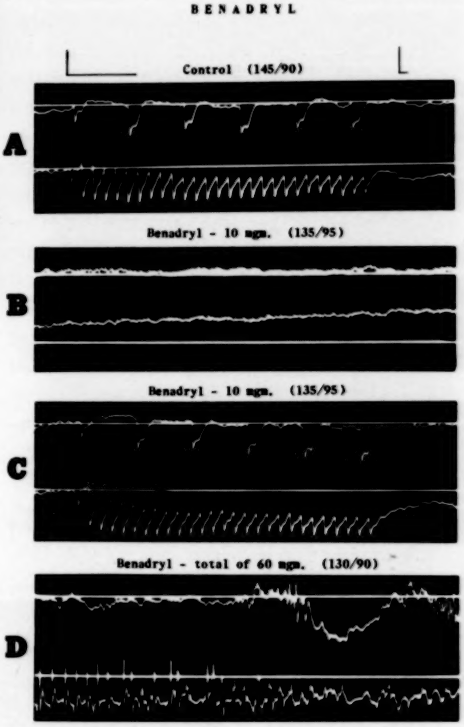

FIG. 5. Effect of benadryl (diphenylhydramine hydrochloride) on SP, serially evoked dendritic spikes and their summed negative after-effect. For A and C stimulus frequencies are 6 and 20 per sec. illustrated in upper and lower traces respectively. A, before benadryl injection. B, positive SP shift following iv. injection of 10 mgm. benadryl, 20 sec. interval separating upper and lower traces. Upper trace follows immediately the end of an injection delivered over a 40 sec. period. C, tracings during the positive shift shown in B. D, convulsoid activity which developed after iv. injection of a total of 60 mgm. benadryl. Note the negative SP shift with the appearance of seizure activity. Calibration symbol, upper right for A, C and D; vertical line 500 μV; horizontal, 20 msec. Calibration symbol, upper left, refers to B; vertical line, 500 μV; horizontal, 1 sec.

with benadryl (compare Figure 5 C with 5 A). However, the slow potential change with 20 per sec. stimulation became significantly augmented, the change usually being noticeable after the initial injection (compare lower trace, Figure 5 C with that of 5 A).

5. *Nitrous Oxide.* These animals were first respired with 100% nitrous oxide for 1 min. and then changed to a mixture of 80% nitrous oxide and 20% oxygen for 10–15 min. Except for the first minute, when the results were complicated by anoxia, no significant change was observed in either the serially evoked dendritic spikes or the summed negative after-effect which develops upon 20 per sec. stimulation. Occasionally a slight reduction in spike height was noted. (Compare Figure 6 B with 6 A). SP was unaffected.

6. *Ether.* A mixture of 10–15% ether and 90–85% oxygen was delivered for 15–30 min. While the blood pressure could drop slightly no significant periods of hypotension occurred. Early in induction (after 2–3 min.) the SP might shift negatively by 300–500 μV and if this occurred there was no return to its former level (Figure 7 B).

Only minor changes were noted in the serially evoked dendritic spikes. The spikes did decrease in amplitude (compare Figure 7 C and D with Figure 7 A) and in some instances the positive after-effect of a single spike was converted into a negative one (compare first response, upper trace, Figure 7 A with first response, upper trace, Figure 7 C). However, the summation of negative after-effect upon 20 per sec. stimulation remained unchanged (compare lower trace, Figure 7 C and D with lower trace Figure 7 A).

7. *Carbon Dioxide.* The animals were respired with a mixture of 10% carbon dioxide and 90% oxygen. Two to three min. after starting the carbon dioxide the blood pressure rose and the pulse rate increased (Figure 8). At this time we observed a significant decrease in the amplitude of both the serially evoked dendritic spikes and of the summed negative after-effect (compare Figure 8 with 8 A) although SP itself remained unchanged. If ventilation

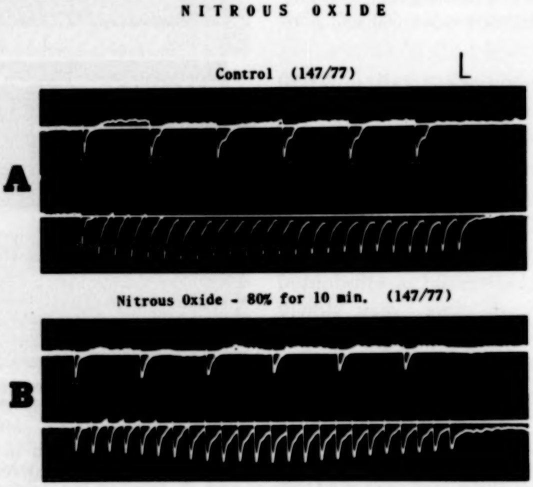

FIG. 6. Effect of nitrous oxide on serially evoked dendritic spikes and their summed negative after-effect. For A and B stimulus frequencies are 5 and 20 per sec., upper and lower traces respectively. A, before starting nitrous oxide anesthesia. B, after 10 min. inhalation of a mixture of 80% nitrous oxide and 20% oxygen. Vertical line of calibration symbol, 500 μV; horizontal one, 20 msec.

at this concentration was continued for three to five min., signs of peripheral vascular collapse developed and all traces of response disappeared. With the appearance of such signs SP shifted positively. The same changes were observed with a 5% mixture, but a longer time was needed for them to develop. In several experiments blood pH was measured before and during carbon dioxide administration. In those cases the pH fell from 7.4 to 7.1, values which were obtained when the blood pressure was elevated and the response diminished in size.

8. *Hypotension by Exsanguination.* Rapid withdrawal of 30–50 cm. quantities of blood usually produced a marked fall in blood pressure with ultimate disappearance of the serially evoked dendritic responses. Figure 9 illustrates the results from a typical experiment. Withdrawal of 30 cm. into a syringe moistened with heparin produced a drop to 50/30 within 1 min. following removal. No immediate change in the response occurred, but 3–4 min. later the serially evoked spikes showed diminished amplitude, the summed negative after-effect with 20 per sec. serial stimulation remaining unchanged (compare Figure 9 B with 9 A). With compensation the blood pressure commenced to return toward a normotensive level. Then an additional 10 cm. was withdrawn, followed by a fall to 45/25. At this blood pressure level the response continued to diminish in amplitude and after 3 more min. had disappeared entirely (Figure 9 C). Thirty cm. of blood were then replaced, the pressure returning to a normotensive level within 1 min. after injection. Recovery of response amplitude lagged behind the restoration of a normotensive pressure, as it did consistently in these experiments, the earliest sign of reappearance of the dendritic spikes occurring 4 min. after restoration of a normotensive status (Figure 9 D). Complete recovery of response amplitude required an additional 3 min. (Figure 9 E). Upon recovery the evoked dendritic spikes might become over-sized temporarily before settling to the pre-exsanguination amplitude.

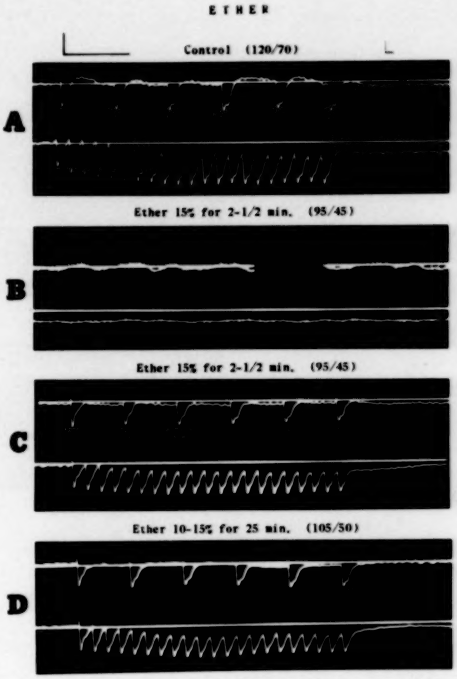

FIG. 7. Effect of ether on SP, serially evoked dendritic spikes and their summed negative after-effect. For A, C and D stimulus frequencies are 5 and 20 per sec. in upper and lower traces respectively. A, before ether anesthesia. B, negative SP shift after 2½ min. inhalation of a mixture of 15% ether and 85% oxygen. Interval of 2 min. 20 sec. separates upper and lower traces; break in the record of upper trace indicates start of ether anesthesia. C, serially evoked dendritic spikes during negative SP shift shown in B, lower trace. D, after 25 min. inhalation of a mixture of 10–15% ether and 85–90% oxygen. Calibration symbol, upper right, refers to A, C and D. Vertical line, 500 μV; horizontal line, 20 msec. Calibration symbol, upper left, refers to B; vertical line, 500 μV; horizontal, 1 sec.

Thus, hypotension produced by exsanguination caused both the dendritic spikes and the summed after-effect of serial stimulation to disappear, the spikes being reduced in amplitude before the change in the summed after-effect was noted.

9. *Hypotension by Arfonad* (Trimethap-

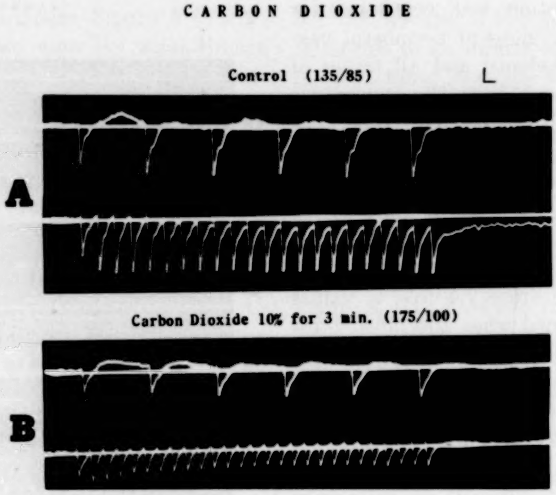

FIG. 8. Effect of CO_2 on serially evoked dendritic spikes and their summed negative after-effect. In A and B stimulus frequencies are 5 and 20 per sec., upper and lower traces respectively. A, before CO_2 inhalation. B, 3 min. after inhalation of a mixture of 10% CO_2 and 90% oxygen. Vertical line of calibration symbol, 500 μV; horizontal, 20 msec.

han). This agent lowers blood pressure by blocking the autonomic ganglia. Following the iv. injection of Arfonad both the serially evoked dendritic spikes and the summed negative after-effect were significantly reduced in size (compare Figure 10 B with 10 A) both elements disappearing completely within 3 min. After 5–6 min. at the reduced level the blood pressure might commence to rise spontaneously; if it did not do so we re-establish a normotensive level by the iv. injection of saline and nor-epinephrine. Even though the blood pressure was restored, however, the response did not recover, and in this respect the Arfonad hypotensive effect differed from that of exsanguination. Figure 10 C shows the complete absence of any response 6 min. after the recovery of a near normotensive level, and in Figure 10 D only a minimal return is evident 30 min. following restoration of normotensive status. Even 1 hour (usual duration of such an experiment) thereafter the response had not fully recovered. Arfonad, then, appears to affect the dendritic parts of the cortical neurones directly as well as indirectly through the hypotension it effects.

10. *Anoxia.* This was produced by respiring the animal with 100% nitrogen. The changes developed rapidly requiring 1½ min. and if great care was not taken to change immediately from nitrogen to oxygen the changes became irreversible and led to death. In a typical experiment after the nitrogen was started the blood pressure rose at first accompanied by a small reduction in the amplitude of the serially evoked spikes. Within the next 30 sec. the SP shifted positively by 300–500 μV and the serially evoked dendritic spikes were further reduced in amplitude; however, no change appeared in the summed negative after-effect (compare Figure 11 B with 11 A). Failure to change to 100% oxygen at this point led to an abruptly appearing negative SP shift and death. However, if oxygen was commenced in time, the blood pressure instead rose to a hypertensive level, followed by another transitory fall and a return to normotensive status within 2–3 min. During recovery the SP shifted

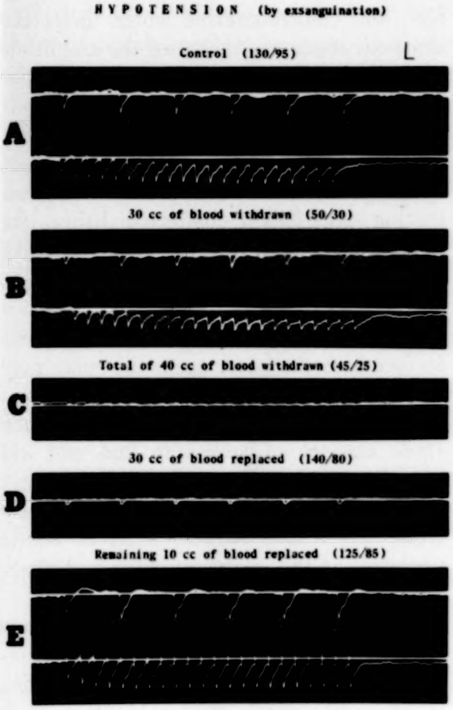

FIG. 9. Effect of hypotension (by exsanguination) on serially evoked dendritic spikes and their summed negative after-effect. In A, B and E stimulus frequencies are 5 and 20 per sec., upper and lower traces respectively. In C stimulus frequency is 20 per sec. and in D, 5 per sec. A, before withdrawal of blood. B, 4 min. after withdrawal of 30 cm. C, record obtained 7 min. after those shown in B and following the removal of an additional 10 cm. of blood. Note complete absence of response in this trace; only shock artifacts of repetitive stimuli being evident. D, record obtained 8 min. after that shown in C and following replacement of 30 cm. blood. *E*, 3 min. after D and after replacement of the remaining 10 cm. Vertical line of calibration symbol, 500 μV; horizontal, 20 msec.

back to the baseline and the spikes regained their normal size (Figure 11 C and D).

DISCUSSION

Many of the changes we describe here would not have been evident using a capacity coupled amplifier and ordinary electrodes. To follow an SP shift and record adequately the slow after-effects of singly evoked responses and their summation upon serial stimulation, required d.c. amplification and non-polarizable electrodes. Otherwise, only the change in amplitude of the dendritic spike would have been evident, the after-effect of the single spike being much abbreviated and evidence of after-effect summation upon serial stimulation largely lost.

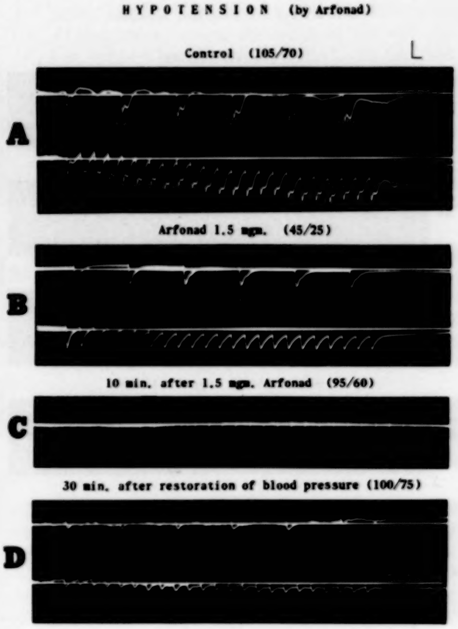

FIG. 10. Effect of hypotension (by Arfonad) on serially evoked dendritic spikes and their summed negative after-effect. In A, B and D stimulus frequencies are 5 and 10 per sec., upper and lower traces respectively, in C, 20 per sec. A, before Arfonad. B, 4 min. after iv. injection of 1.5 mgm. Arfonad. C, 10 min. after B and 4 min. following iv. injection of 20 cm. saline to restore normotensive status. D, 30 min. after C. Vertical line of calibration symbol, 500 μV; horizontal 20 msec.

We observed the combination of reduced amplitude of serially evoked spikes and augmentation of the summed negative after-effect only in the case of nembutal. Specifically this augmentation did not develop with other agents which also reduced the height of the dendritic spikes, nor did it appear with hypotension or with anoxia. Thus, because the same situation did not

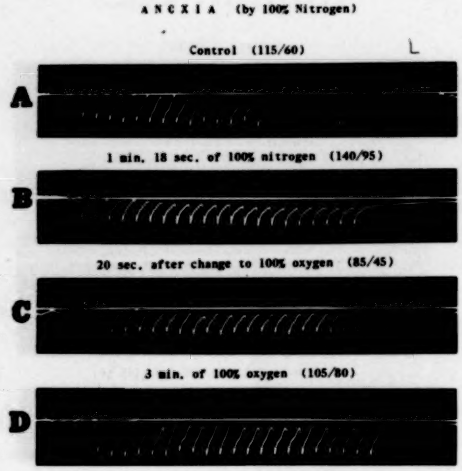

FIG. 11. Effect of anoxia (100% nitrogen inhalation) on serially evoked dendritic spikes and their summed negative after-effect. In all traces stimulus frequency is 20 per sec. A, before nitrogen inhalation. B, 1 min. 18 sec. after start of 100% nitrogen. At this time SP has commenced to shift positively (see text for details). C, 20 sec. after start of 100% oxygen inhalation. D, 3 min. after start of oxygen inhalation. Vertical line of calibration symbol, 500 μV; horizontal, 20 msec.

prevail in all instances, we cannot attribute the augmentation of the summed after-effect in the case of nembutal to slowed post-activity repolarization of dendrites incident to neuronal depression (4). That had been our theoretical assumption when we undertook this study.

Much of the evidence we present here, in fact, indicates that the negative dendritic spike and the after-effect of like polarity are separate neural processes. For example, the serially evoked dendritic spikes were more sensitive to hypotension and to anoxia than was the concurrent summation of negative after-effect, no change in that after-effect being evident at a time when there was already a significant reduction in spike height by exsanguination (Figure 9 B) or by ventilation with 100% nitrogen (Figure 11 B). Furthermore, except for nitrous oxide, which affected neither the serially evoked dendritic spikes nor their summed after-effect, and chlorpromazine which under the same circumstances increased the amplitude of both, the agents affected the two components differentially. Procaine had little effect upon the serially evoked spikes, but caused the summed after-effect to disappear. Benadryl increased the summed after-effect leaving spike height unaffected. Ether, like hypotension and anoxia, reduced spike height before it altered summed after-effect, and nembutal, described previously, reduced spike height while augmenting after-effect. In fact with the latter agent the spike may fail to appear entirely while the slow negatively augments and shows summation (note absence of spikes to 2nd and 3rd stimuli while negative after-effect increases in size, Figure 2 C, lower). Taken collectively the results suggest different neural processes as the origins of spike and after-effect of the same polarity.

None of the results of these experiments suggest an origin for the positive after-effect. However at present we are exploring other agents which appear to augment it selectively.

SUMMARY

Using d.c. recording and non-polarizable electrodes in the cat we tested the effect of a variety of agents upon the dendritic spike of the cerebral cortex and its slow potential aftermaths, and especially of the summed negativity which results from serial stimulation. Changes in steady potential (SP) of the cortex were also followed. Nembutal, exemplifying barbiturate anesthetics, and nitrous oxide and ether the volatile ones, were studied, as were chlorpromazine, benadryl, and procaine. The effects of the volatile anesthetics were controlled by the inhalation of nitrogen, oxygen and carbon dioxide. To eliminate a rapid drop in blood pressure as a causative factor in some of the changes reported, separate experiments were carried out in which the effects of exsanguination and Arfonad (a ganglionic blocker) were tested.

A variety of effects were obtained with different agents, each repeatable. The evidence supports different neural origins for the negative dendritic spike and its after-effect of like polarity. It does not suggest the origin of the positive after-effect.

REFERENCES

1. Adrian, E. D. The spread of activity in the cerebral cortex. J. Physiol., **88:** 127–161, 1936.
2. Chang, H. T. Dendritic potential of the cortical neurones produced by direct stimulation of the cerebral cortex. J. Neurophysiol., **14:** 1–23, 1951.
3. Clare, M. H. and Bishop, G. H. Properties of dendrites; apical dendrites of the cat cortex. EEG Clin. Neurophysiol., **7:** 85–98, 1955.
4. Goldring, S., O'Leary, J. L. and Huang, S. H. Experimental modification of dendritic and recruiting processes and their D.C. after-effects. EEG Clin. Neurophysiol., **10:** 663–676, 1958.
5. Landau, W. M. An analysis of the cortical response to antidromic pyramidal tract stimulation in the cat. EEG Clin. Neurophysiol., **8:** 445–456, 1956.
6. O'Leary, J. L. and Goldring, S. Changes associated with forebrain excitation processes: D.C. potentials of the cerebral cortex. Handbook of Physiology, published by American Physiological Society. In press.

THE EFFECTS OF CHLORPROMAZINE AND SECOBARBITAL ON THE C.P.T.[1]

ALLAN F. MIRSKY,[2] PH.D., DANIEL W. PRIMAC, M.S.[3]
AND RICHARD BATES, B.S.[4]

This study is the second in a series of investigations of the effects of centrally-acting drugs on the Continuous Performance Test (C.P.T.) (10) which was designed to measure deficit in sustained attention.

In the first study of this series, Primac, Mirsky and Rosvold (9), working with 'clinical' doses of a number of centrally-acting drugs, showed that chlorpromazine impairs the ability of normal subjects to perform on the C.P.T. High doses of the drug (200 mg.) produce even more impairment than low doses (100 mg.). Other centrally-acting drugs, including L.S.D.-25 (lysergic acid diethylamide), meperidine and secobarbital sodium were in the dosages used without significant effect on the C.P.T.

The deficit produced by chlorpromazine on the C.P.T. was interpreted as an impairment in alertness or vigilance brought about by depression of the subcortical activating system. Supporting this view is the fact that the majority of electrophysiological studies reviewed by Primac *et al.* (9) suggest that system to be the primary locus of effect of chlorpromazine. While the activating system has been implicated in the effects of meperidine, the studies reviewed by Wikler (13) suggest that there is only a minimal effect of this drug on the activating system. There is considerable evidence, however, (1, 13) indicating that barbiturates act on the brain stem; hence the lack of effect of secobarbital on the C.P.T. is somewhat difficult to reconcile with the interpretation that the effect of chlorpromazine on the C.P.T. is due to depression of the subcortical activating system. Accordingly, the suggestion that the C.P.T. is a measure of the integrity of the subcortical activating system is weakened by the lack of evidence of any effect of secobarbital. The subjects in the previous study, however, were tested between three and four hours after drug administration. Such an interval is within the period of maximum activity of chlorpromazine but possibly not of secobarbital, since the latter drug is considered to be a short-acting barbiturate (7).

A rather different interpretation of the results of the previous study is suggested by some data obtained by Carlson (2). Carlson measured pupil size after drug administration using the same subjects and drugs reported by Primac *et al.* (9) and Kornetsky *et al.* (8). Only chlorpromazine had a significant constricting effect on the pupil. Since only chlorpromazine significantly impaired C.P.T. performance in these subjects, the possibility exists that the deleterious effect of this drug is due to a reduction of the amount of light in the visual field, rather than to any central effect.

The present investigation was designed to gather information concerning the effect of secobarbital on the C.P.T. and information which would be relevant to the alternative interpretation of the effect of chlorpromazine on the C.P.T. suggested by Carlson's data. Two experiments are presented; the first was conducted in an attempt to confirm the previous finding that chlorpromazine interferes with performance on the C.P.T. and secondly to provide information on the extent to which chlorpromazine has a different effect on different subjects. In the previous study (9) it was not possible to measure this since the latin square design which was used did not permit the testing of drug x subject interaction. The second experiment was designed specifically to measure the effect of secobarbital on the C.P.T. Subjects were tested a number of times at successive intervals following administration of this drug.

---

[1] The authors wish to express their appreciation to Dr. Seymour Geisser, Experimental Statistics Branch, National Institute of Mental Health, for assisting in the design of this study and in the analysis of the data, and to Mr. Arthur M. Townsend III for assistance in testing some of the subjects, and to the staff of the Laboratory of Clinical Sciences for their assistance and cooperation in this research.

[2] Section on Animal Behavior, Laboratory of Psychology, National Institute of Mental Health, National Institutes of Health, Bethesda, Maryland.

[3] Now in area of the Chief, Laboratory of Psychology, National Institute of Mental Health.

[4] Now at McGill University School of Medicine, Montreal.

GENERAL METHODS

Apparatus. The C.P.T., used in both experiments, consists essentially of a revolving drum on which are mounted two series of 31 letters. One or the other series of letters may be brought into view by adjusting a screen at the front of the apparatus. One series constitutes the *X* task, the other series, the *AX* task. The *X* task, which is always given first, requires that the subject press a response key every time the letter *X* appears; the *AX* task requires the subject to press the key for every *X* that follows immediately after the letter *A*.

C.P.T. Scores. A ten minute record (provided by 20 revolutions of the drum) of the subject's performance on each task is scored in two ways: 1) the relative score, defined as the number of correct responses divided by the total number of responses the subject has made, and 2) the absolute score, defined as the number of correct responses divided by the total number of correct responses possible. Since there are 160 *X*s presented in the *X* task, there are 160 possible correct responses. Similarly, there are 120 possible correct responses on the *AX* task. Thus the C.P.T. yields four measures: *X*-Relative, *X*-Absolute, *AX*-Relative and *AX*-Absolute.

EXPERIMENT 1. METHODS

Subjects. Twelve normal individuals, five male and seven female, whose mean age was 21.1 and whose mean full scale I.Q. was 115.8, served as subjects.

Procedure and design. Subjects were tested on the C.P.T. immediately after being tested by Kornetsky and Humphries (8) on the battery of procedures used by these investigators. Thus, the subjects took the C.P.T. an average of two hours and forty-eight minutes after administration of either 100 mg. or 200 mg. of chlorpromazine hydrochloride or secobarbital sodium, or a placebo. Neither the subjects nor the experimenters knew whether a drug or a placebo had been administered on a particular day. The subjects were administered the drugs (or placebo) orally in identical gelatin capsules. At least forty-eight hours separated each administration. Each subject was administered each dose of the two drugs twice and a placebo twice for a total of ten treatments. The treatments were administered according to a balanced design which permitted partition of the total variance into the following components: Subjects, drugs, error, and the interaction of subjects with drugs (independent of error). In addition to being tested on the C.P.T., each subject was asked to report how he felt at the time of testing. A report of "very sleepy" or behavioral indication of extreme drowsiness was assigned a rating of two. A report or behavioral indication that the subject was moderately sleepy was assigned a rating of one, while a report or indications that the subject was experiencing no sleepiness at the time of testing was assigned a rating of zero. There was invariably close correspondence between the subjects' reports and the behavioral indications of the degree of drowsiness.

EXPERIMENT 1. RESULTS

The analyses of variance (12) of the several C.P.T. scores revealed that in each case the variance due to drugs and to subjects

TABLE 1

Means of the C.P.T. Measures in the Several Treatments, Experiment 1

C.P.T. Measures	Placebo	Chlor. 100 mg.	Chlor. 200 mg.	Secob. 100 mg.	Secob. 200 mg.
X-Relative	95.54	86.10†	75.09*	96.96	95.34
X-Absolute	95.47	81.82†	65.47*	96.90	95.08
AX-Relative	95.42	87.94	74.40*	93.98	93.51
AX-Absolute	93.71	81.91†	57.43*	91.91	92.22

* differs from placebo at $p < .005$

† differs from placebo at $p < .05$

TABLE 2

Mean Sleepiness Ratings (2 replications combined), Experiment 1

Treatment	Placebo	Chlor. 100 mg.	Chlor. 200 mg.	Secob. 100 mg.	Secob. 200 mg.
Mean Rating	0.21	1.15	1.60	0.67	0.64
p of Diff. from placebo	—	.033	.054	.254	.113

was highly significant ($p < .001$). In only one case (*AX*-Absolute) was the variance due to the interaction of subjects with drugs significant ($p < .05$).

Table 1 presents the pooled means (the combined mean of the two administrations) of the several C.P.T. scores for each of the doses of the drugs. Comparisons of the treatment means with the placebo means (5) revealed that all chlorpromazine treatment means differed from their respective placebo means at $p < .05$ (except that for *AX*-Relative at 100 mg.). None of the secobarbital treatment means differed from their respective placebo means. The 200 mg. chlorpromazine score was significantly lower than the 100 mg. score at $p < .05$ in each case (11).

To check on the possibility that the deficit produced by chlorpromazine was due simply to the reduction of the amount of light in the visual field (as suggested by Carlson's data on pupil size) ten subjects were tested on the C.P.T. first under normal conditions and second while wearing a pair of tightly-fitted goggles which excluded all light save that which came through a 1.5 mm. pinhole aperture in each lens of the goggles. This provided in effect an artificially constricted pupil which reduced the amount of light in the field of vision and thus simulated the effect of the chlorpromazinized pupil (the smallest individual chlorpromazinized pupil found by Carlson (3) measured 1.6 mm.).

In no instance was there a reduction in the average C.P.T. score produced by the artificial pupil. The difference between the normal vision and artificial pupil conditions was of the same order as the difference between the two placebo conditions in Experiment 1.

Table 2 presents the mean sleepiness ratings, with the scores from the two replications of each treatment combined into one treatment mean. Each of the four treatment ratings was compared with the placebo rating by the one-tailed sign test (4). Chlorpromazine administration was accompanied by a significant increase in sleepiness scores, whereas the secobarbital sleepiness scores did not differ from those of the placebo. The scores with the 200 mg. dose of chlorpromazine differed from those with the 100 mg. dose at $p = .054$. The effect of the several treatments on sleepiness scores was in general similar to the effect on C.P.T. scores, although the differences among the latter tended to reach higher levels of significance.

EXPERIMENT 2. METHOD

Subjects. Eight normal males, whose mean age was in the early 20s and all of whom were of at least average intelligence, served as subjects.

Procedure and design. All subjects were administered orally a single 200 mg. dose of secobarbital sodium and tested for seven minutes on the AX task of the C.P.T. during successive 25-minute intervals. One test was given prior to drug administration and seven following it, so that the eight tests covered the period from 20 minutes pre-drug to two hours and fifty minutes post-drug. In addition to these measures, an informal record was kept of the subjects' reported or grossly observable drowsiness or sleepiness during the several tests.

EXPERIMENT 2. RESULTS

Table 3 presents the median scores and ranges in terms of the absolute percentage of correct AX responses, for the eight subjects during the eight test periods. The scores obtained during the second and third drug tests (*i.e.*, at 30 to 50 minutes and 55–75 minutes post-drug) are significantly lower ($p < .05$) than the pre-drug scores (t test for correlated measures). The scores of the first drug test (5–25 minutes post-drug) and of the fourth drug test (80–100 minutes post-drug) do not differ significantly from the pre-drug scores. This suggests that 200 mg. of secobarbital produces its maximum effect on the C.P.T. between 30 and 75 minutes post-administration, but that before and after this period the effect would not be picked up as significant. For purposes of comparison of effects, it may be noted that the mean AX-Absolute performance of the subjects in Experiment 1, under 200 mg. of chlorpromazine, was 57.4% at three hours after administration (the point in time post-administration when the subjects in Experiment 1 were tested on the AX task).

With respect to the informal record of the subjects' grossly observable or reported effects of the drug, there seemed to be good correspondence between the degree of deficit on the AX task and the amount of sleepiness of the subject. Between 30 and 75 minutes post drug administration there were frequent reports, as, "I can't concentrate", or "I feel kind of sleepy." However, at or near two hours post drug, the subjects reported, "I'm feeling better" or, "Not sleepy now", or "I feel all right now." These results may be compared with the results of the sleepiness ratings in Experiment 1. Nearly three hours after administration of 200 mg. of secobarbital the mean sleepiness ratings of the subjects did not differ from those under the placebo, which is consistent with the informal observations in this experiment. In contrast, however, the mean sleepiness rating three hours after administration of 200 mg. of chlorpromazine was somewhere between moderately sleepy and very sleepy.

TABLE 3

Medians and Ranges of AX-Absolute Percentage Correct Responses, Experiment 2

Successive Tests	Median	Range
Pre-drug	92.2	79.7-98.8
1 (5′-25′)	86.9	79.7-98.8
2 (30′-50′)	74.4*	13.0-100
3 (55′-75′)	80.9*	41.7-92.8
4 (80′-100′)	88.1	44.0-97.6
5 (105′-125′)	89.9	60.7-95.2
6 (130′-150′)	91.0	69.0-96.4
7 (155′-175′)	88.1	73.8-98.8

* differ significantly from pre-drug scores at $p < .05$ (one-tailed test)

DISCUSSION

The results of Experiment 1 confirm earlier findings that chlorpromazine impairs performance on the C.P.T. with the impairment becoming more severe as the dose of chlorpromazine is increased. The lack of significant interaction between drugs and subjects in this experiment suggests that chlorpromazine affects all subjects uniformly.

That chlorpromazine produces its effects by interference with central rather than with peripheral mechanisms is suggested by the findings of the artificial pupil procedure. Although this procedure does not control for the other peripheral affects of chlorpromazine, it rules out the possibility that the C.P.T. deficit is simply a consequence of less light in the field of vision. Other evidence that the chlorpromazine effect is unrelated to pupillary constriction is provided by Carlson (3) who found no significant correlation between the C.P.T. scores of *S*s under 200 mg. of chlorpromazine and the amount of pupillary constriction produced by the drug.

The results of Experiment 2 indicate that secobarbital does have a measurable depressing effect on the C.P.T., if measured within the period of its maximum activity. Significant decrements in the performance of the subjects were observed in the tests administered between 30–50 and 55–75 minutes post drug but at no other time. The failure to demonstrate a significant impairment with secobarbital in the study by Primac *et al.*, and in Experiment 1 of the present study, is apparently due to the fact that the testing was conducted too late, *i.e.*, four and three hours, respectively, after the administration of the drug. Thus, no significant effect of secobarbital would have been observed in these experiments, since according to the results of Experiment 2, the effect of secobarbital on the C.P.T. is of short duration and does not persist beyond 75 minutes post-administration, approximately. In contrast with this effect of relatively short duration, the deficit produced by chlorpromazine is quite marked and statistically significant three hours (Experiment 1) and four hours (Primac *et al.*) after administration. As can be seen from Table 3, the deficit produced by 200 mg. of secobarbital, even at the peak of its effect, is of much smaller magnitude than the deficit produced by 200 mg. of chlorpromazine three hours after administration. Obviously, these two 'clinical' doses of the two drugs are not equivalent in terms of the duration and degree of the behavioral deficit they produce. Possibly, an effect similar in duration and degree to that of chlorpromazine could be produced by a long-acting barbiturate such as phenobarbital.

An interpretation of the long-lasting effect of chlorpromazine and the brief effect of secobarbital on the C.P.T. (Experiments 1 and 2) is aided by an examination of the pattern of mean sleepiness ratings gathered in the course of Experiment 1 and the informal observations of the subjects' sleepiness in Experiment 2. In general, subjects were sleepier following chlorpromazine administration than following administration of the placebo, with the high dose of chlorpromazine producing more sleepiness than the low dose. The sleepiness ratings following secobarbital administration in Experiment 1 did not differ from those obtained with the placebo. This would be expected in view of the results of Experiment 2, since in Experiment 1 the secobarbital effect was being measured at approximately three hours after administration. When sleepiness is measured near the peak effect of secobarbital (Experiment 2) then there is again a correspondence between degree of sleepiness and C.P.T. performance. The close correspondence between the C.P.T. scores and the sleepiness ratings suggests that the C.P.T. is measuring the extent to which an individual can maintain continuous wakefulness, a function presumably mediated by the subcortical activating system.

The impairment produced by chlorpromazine and by secobarbital may be related to the known depressant effect of these drugs on the reticular and thalamic portions of the activating system. The brief effect produced by secobarbital is consistent with the classification of this drug as a short-acting barbiturate (7). Therefore, it seems reasonable to propose that pharmacological agents which act to disturb the subcortical activating system will produce impairment in C.P.T. performance.

SUMMARY

Two experiments were conducted in an investigation of the effects of chlorpromazine on the Continuous Performance Test (C.P.T.). Normal volunteer subjects were used in all three experiments.

In Experiment 1, low and high doses of chlorpromazine, secobarbital or a placebo were administered to 12 subjects. Each subject was administered each dose of the two drugs twice and the placebo twice. The testing was conducted between two and three hours after administration. Chlorpromazine impaired performance on the C.P.T. with

the impairment becoming more severe as the dose of the drug was increased; secobarbital was without effect on the C.P.T. The absence of significant drug x subject interaction suggested that these effects were uniform for all subjects. Chlorpromazine administration was also accompanied by a significant increase in ratings of the subjects' sleepiness, whereas the secobarbital sleepiness scores did not differ from the placebo. Restricting the amount of light in the visual field (pupillary constriction is a side effect of chlorpromazine) does not impair C.P.T. performance.

In Experiment 2, eight subjects were tested on the C.P.T. immediately before and during seven successive 25-minute intervals following administration of 200 mg. of secobarbital. Significant impairment on the C.P.T. was evident between 30 and 75 minutes post drug administration but at no other time. This suggested that in the previous studies (including Experiment 1) in which the effect of secobarbital was measured the testing was conducted after the significant effect of this drug on the C.P.T. had passed.

It was proposed that pharmacological agents, such as secobarbital and chlorpromazine, which act to disturb the subcortical activating system will produce impairment in C.P.T. performance.

REFERENCES

1. Bradley, P. B. and Key, B. J. The effect of drugs on arousal responses produced by electrical stimulation of the reticular formation of the brain. Electroencephalog. & Clin. Neurophysiol., **10:** 97–110, 1958.
2. Carlson, V. R. Individual pupillary reactions to certain centrally acting drugs in man. J. Pharmacol. & Exper. Therap., **121:** 501–506, 1957.
3. Carlson, V. R. Personal communication, 1958.
4. Dixon, W. J. and Mood, A. M. The statistical sign test. J. Am. Statist. A., **41:** 557–566, 1946.
5. Dunnett, C. W. A multiple comparison procedure for comparing several treatments with a control. J. Am. Statist. A., **50:** 1096–1121, 1955.
6. Edwards, A. L. *Experimental design in psychological research.* Rinehart, New York, 1950.
7. Goodman, L. S. and Gilman, A. *The pharmacological basis of therapeutics.* Macmillan, New York, 1956.
8. Kornetsky, C. and Humphries, O. In press, J. Ment. Sc., 1958.
9. Primac, D. W., Mirsky, A. F. and Rosvold, H. E. Effects of centrally acting drugs on two tests of brain damage. A.M.A. Arch. Neurol. & Psychiat., **11:** 328–332, 1957.
10. Rosvold, H. E., Mirsky, A. F., Sarason, I., Bransome, E. D. Jr. and Beck, L. H. A continuous performance test of brain damage. J. Consult. Psychol., **20:** 343–350, 1956.
11. Scheffé, H. A method for judging all contrasts in the analysis of variance. Biometrika, **40:** 87–104, 1953.
12. Snedecor, G. W. *Statistical Methods*, Iowa St. Coll. Press, Ames, Iowa, 1946.
13. Wikler, A. Sites and mechanisms of action of morphine and related drugs in the central nervous system. Pharmacol. Rev. **2:** 435–506, 1950.

In the past few years there has been a great deal of controversy concerning possible relationships of schizophrenia and experimentally induced schizophrenic-like symptomatology to the production, presence and action of adrenochrome. A number of investigators have not been able to obtain evidence of its spontaneous production in the organism. In addition, no agreement has been reached as to whether or not its injection products so-called mental effects—the reports vary widely in this regard. There is agreement, however, that certain breakdown products of adrenaline do have definite effects on brain tissue and on the organism as a whole. Since this entire question relates to various fundamental aspects of psychosis, the Editors have thought it of value to publish this discussion by two of the foremost proponents of the adrenochrome hypothesis. It is hoped that it may serve to encourage further discussion and investigation in this most controversial area.

THE ADRENOCHROME MODEL AND SCHIZOPHRENIA[1]

A. HOFFER, M.D.[2] AND H. OSMOND, M.D.[3]

INTRODUCTION

So far medical research has met with little success in its attempts to discover biological mechanisms underlying schizophrenia. It is sometimes forgotten by those who glibly refer to schizophrenia as a faulty psycho-social reaction, a way of life, etc., that if their contentions were true, then it would be outside medicine and our speculations would be simply impertinent. Psychiatrists have seldom been prepared to relinquish this group of major illnesses but, baffled by its complexity, they have resorted to the dubious ruse of proclaiming under a specious holism inherited from Adolph Meyer, that all factors are or may be of equal importance. This has allowed them to avoid the danger of being wrong while ensuring that they will never be right. Unfortunately as a prescription for scientific research, this is poisonous because the researcher is bound to select those variables which he considers more important, or if not more important, at least more susceptible to study. A medical researcher must be alert to factors which are likely to be changed for the better by means available to his profession. A medical man may reasonably be expected to consider first the means of medicine and surgery.

A theory of schizophrenia must not only account for what we already know about it from both clinical observation and investigation; but it must also be testable. Untested theories however attractive are not of scientific interest. The attempt to define schizophrenia chemically has bogged down due to the fact that biochemists are unable to isolate or identify toxic (and presumably psychotomimetic) substances unless they know what they are looking for (**12, 13**). It appears that this limitation of chemistry has not been fully appreciated by psychiatrists or, if appreciated, no one recognized that an essential task was to define the sorts of substances which should be got as accurately as possible.

The adrenochrome hypothesis of schizophrenia was developed by the Saskatchewan group (**43**) to remedy this and to guide biochemists. Chemicals were studied which were termed hallucinogenic and more recently have been called psychotomimetic.

---

[1] Research supported by National Health Grants, Ottawa, the Rockefeller Foundation, New York, and the Saskatchewan Committee on Schizophrenia Research.

[2] University Hospital, Saskatoon, Saskatchewan, Canada.

[3] Saskatchewan Hospital, Weyburn, Saskatchewan, Canada.

Adrenochrome and adrenolutin might now be called schizogenic substances. When they are given they produce an experience which more closely resembles early schizophrenia than that induced by LSD-25 (hereafter LSD) or mescaline (34, 81).

We have studied experimental or model psychoses in humans which we consider resemble schizophrenia more closely than they resemble the toxic psychosis. The compounds which to our way of thinking do this were LSD etc. We consider the crucial difference between the toxic and schizophrenic-like model psychosis lies in the presence of clouding and confusion in the former and their absence in the latter. Of the substances we included among the hallucinogens, three were indoles and the fourth, mescaline, may be indolized in vivo (27). Mescaline is very similar in structure to epinephrine (43). We therefore examined body fluids for indolic substances derived from epinephrine. This led to a study of adrenochrome (43) and later adrenolutin (35, 36, 37, 38). Since then bufotenine (17) and dimethyl and diethyl tryptamine (95) have been reported to be psychotomimetic. An additional psychotomimetic is trimethoxy phenyl amino propane (76). Ololuiqui (69) contains unknown active substances and the Mexican hallucinogenic mushroom contains active indoles (47).

Bowel putrefaction and tryptophan metabolism is, of course, a major source of indoles in the body but we have not examined these because they do not seem to bear much relationship to the anxiety and tension which often appears to be the trigger to schizophrenia. This does not mean that they may not sometimes play an important role in the geneses of the disease.

It has been suggested by some colleagues that a simple demonstration that the body fluids of schizophrenic people contain more adrenochrome and adrenolutin than do those of normal people or mentally ill suffering from other illnesses will substantiate our hypothesis. In our view, the matter is not so simple. Four different steps are necessary to achieve this end: (1) adrenochrome, adrenolutin and/or other immediate derivatives of them must not only be present but must be shown to have some relationship to schizophrenia, (2) these epinephrine derivatives must reproduce some of the psychological and clinical features of schizophrenia, (3) adrenochrome and adrenolutin must reproduce some of the biochemical features of schizophrenia, (4) the hypothesis must be useful in developing new treatments.

We shall discuss these crucial experiments using data already recorded and presenting new data from our own laboratory which relates to them.

BIOCHEMICAL DIFFERENTIATION OF SCHIZOPHRENIC FLUIDS FROM NON-SCHIZOPHRENIC FLUIDS

INTRODUCTION

One advantage of the early indole theory of mental disease first proposed was simplicity (52). The bowel toxins were supposed to accumulate in the blood especially of the constipated. With greater sophistication in medicine generally and the development of biochemistry, this was slowly disproved. Toxic hypotheses implicated vast groups of compounds such as amines, nitrogenous compounds, etc. and so were impossible to test. Later this was narrowed to indoles but this still leaves tens or even hundreds of compounds. If, however, one decides to look for indoles related to epinephrine and trytophan, then the problem becomes much smaller.

From our knowledge of the properties of adrenochrome and adrenolutin, we should be able to predict the sorts of differences there would be between body fluids taken from schizophrenic patients and those of other people. If we then add adrenochrome and adrenolutin to normal serum and this then becomes more like schizophrenic serum, this will suggest that something similar is

TABLE 1

Toxicity of Normal and Schizophrenic Plasma and of Some Psychotomimetic Substances

Assay	Body Fluids		Chemicals		
	Normal	Schizophrenic	Adrenochrome	Mescaline	LSD
L strain fibroblasts	No	Yes	Yes	?	No
Rat climbing	No	Yes	Yes	Yes	Yes
Spore germination	No	Yes	Yes	Yes	Yes
Spider web	No	Yes	Yes	Yes	Yes
Pigeon catatonia	No	Yes	Yes	?	No

present in schizophrenic serum. We have therefore examined the effect of schizophrenic fluids in biological assays compared to psychotomimetic substances and to search them for the presence of indoles related to epinephrine.

BIOLOGICAL ASSAYS

L-strain fibroblast toxicity. In this test, serum taken from patients in the morning before breakfast is added to tissue cultures of L-strain fibroblasts (18, 19). Schizophrenic serum markedly inhibits cell growth whereas this occurs much less frequently when normal serum is used (20). Blood serum from patients undergoing surgery is also toxic but that from neurotic patients is similar to the normal group. Some neurotic patients high in toxicity show many schizophrenic features (20).

Adrenochrome and adrenolutin are antimitotic (54) and thus toxic for some cells. However, Fedoroff's work (19) shows that at least two factors are involved in the toxicity test which cannot with certainty be ascribed to adrenochrome and adrenolutin alone.

Rat climbing test. When trained climbing rats are injected with a standard quantity of schizophrenic serum, performance is greatly impaired (104). Normal serum does not have this effect. Serum from about 90 per cent of schizophrenic patients have this property and from only about ten per cent of normals. This distribution of toxicity is very similar to that found by the fibroblast test.

LSD (103) and adrenochrome (105) also impair rat performance and certain urinary derivatives from schizophrenics have a similar effect (104).

Spore germination test. When suitable extracts from the serum or urine of schizophrenics are added to the spore of certain fungi their germination is inhibited (89, 90). Similar extracts from normal people do not do this. From a variety of substances tested so far, LSD, mescaline and adrenochrome semicarbazone and adrenochrome inhibited germination in concentrations of one in a million. Serotonin and bufotenine inhibit germination at concentrations of one in a thousand. Schizophrenic serum contains substances which are more toxic for these spores than any chemical tested so far.

Spider web test. When the extract from schizophrenic urine is fed to certain spiders, there is an impairment in the quality of the web (80). Mescaline, LSD and adrenochrome are also very active in this respect whereas adrenochrome semicarbazone does not distort the web (106).

Catatonizing effect on pigeons. Schizophrenic bile produces a form of disinterest and indifference in pigeons which has been called catatonia (3). Normal bile has no effect. The administration of 5 to 10 mg. per pigeon of adrenochrome and of adrenolutin produces a similar reaction (40). LSD (½ mg.) has little effect on pigeon behavior (6).

The results of the five biological tests are summarized in Table 1.

On these tests, adrenochrome has very similar effects to those of extracts of schizophrenic fluids. Mescaline and LSD do not resemble the schizophrenic so closely. The only substances tested so far which are likely to be present in the body are adrenochrome and adrenolutin. But they alone are required in dosages larger than one would expect to find in serum. However, schizophrenic serum contains taraxein (29) which can markedly potentiate the activity of adrenochrome and adrenolutin (65) and thus small quantities will be more effective than larger quantities of pure compound.

ADRENOCHROME LEVELS AND SCHIZOPHRENIA

Biological assays often precede more accurate biochemical methods. We have developed a method for measuring adrenochrome in plasma, cerebrospinal fluid and urine (73). The method is specific for noradrenochrome and adrenochrome. Work in our laboratory (75) suggests that noradrenochrome is not present in the body.

The method is based upon the conversion of adrenochrome to a zinc complex of adrenolutin. This is extracted into acetone and its fluorescence determined. Recovery experiments are satisfactory and reproducibility is good.

The mean plasma adrenochrome values for a series of fifty-seven normals was 56 μg/liter, ranging from 18 to 106. Means for thirty-three schizophrenics, sixteen depressed patients and ten alcoholics were 55, 50 and 52 μg./liter. These values do not differ from those found in plasma drawn from normal subjects.

Adrenochrome levels in plasma compared to cerebrospinal fluid are shown in Table 2.

The levels in cerebrospinal fluid of medical and surgical patients is nearly the same as for plasma, *i.e.* about 65 μg/liter with a ratio close to 1.0. With schizophrenic patients, this ratio is closer to 2.0. This data indicates that the brain may be the source of adrenochrome and that there is more in the schizophrenic brain. High oxygen tension increased brain adrenochrome (26).

TABLE 2

Adrenochrome Levels in Plasma and Cerebrospinal Fluid, μg/liter

Group	N	Plasma	CSF	Ratio
Schizophrenic	15	43	82	1.9
Possibly Schizophrenic	5	35	104	1.2
Depression or Anxiety	7	50	47	0.9
Means for Normals		66	68	1.0

ADRENOCHROME TOLERANCE

Since there was no difference between various groups of patients and as adrenochrome is very rapidly destroyed by blood in vitro (44) and in vivo (22, 23, 41), the ability of groups of patients to destroy adrenochrome was determined. The patients were injected with 10 mg. crystalline adrenochrome (28) intravenously and plasma adrenochrome determined at one quarter, one half and one hour after injection. The results are shown in Table 3.

Much of the injected adrenochrome is very rapidly converted into adrenolutin and the fluorescence of the acetone blank is markedly increased.

The plasma adrenochrome values fifteen minutes after injection with adrenochrome are between three and four times the pre-injection value for the schizophrenic and possibly schizophrenic group. After one hour, the values are twice as high in the first group but have returned to normal in the second. In the schizophrenics who had

TABLE 3

Tolerance for Ten Milligrams Intravenous Adrenochrome

Group	Treatment	N	Initial Level	Percent Change at		
				15 min.	30 min.	60 min.
Schizophrenic	None	6	43	+280	+75	+107
Possible Schizophrenic	None	5	68	+220	+10	−6
Schizophrenic	Niacin	5	50	+100	0	−44
Not Schizophrenic	None	9	51	+170	−45	−23
Not Schizophrenic	35 μg LSD-25*	6	43	+260	+100	+67
Not Schizophrenic	100 μg LSD-25*	2	46	—	—	+170

* 2 hours before adrenochrome tolerance.

received large quantities of nicotinic acid before the test, little increase was noted and after one hour levels had decreased below the initial level. A similar pattern is seen for non-schizophrenic subjects.

The schizophrenics were not able to destroy adrenochrome as rapidly as non-schizophrenic subjects. However, after nicotinic acid treatment the ability to destroy adrenochrome was increased even though these patients were not clinically improved. The schizophrenic adrenochrome tolerance curve resembles that in normals pretreated with LSD (41). Pretreatment with brom LSD does not produce abnormal adrenochrome tolerance curves.

CONVERSION OF ADRENOCHROME TO ADRENOLUTIN

The adrenochrome is quickly converted into adrenolutin (37) and leuco adrenochrome. Adrenolutin is readily extracted by the acetone which shows a distinct peak at the specific fluorescence area for adrenolutin. The increase in the acetone blank over the pre-injection value is therefore a measure of the amount of adrenochrome converted into adrenolutin. The blank went up 3 fluorescence units for eleven schizophrenic patients one half hour after injection and 4 units for eleven non-schizophrenic patients. These changes are not significantly different. However, at this time, non-schizophrenics had destroyed all the injected adrenochrome whereas in schizophrenics the levels were still elevated. This indicates that schizophrenics convert a greater proportion of adrenochrome into adrenolutin.

We examined the in vitro conversion of adrenochrome into adrenolutin by adding adrenochrome to plasma buffered at pH 4.0. Disappearance of adrenochrome was measured by the decrease in optical density at 480 $m\mu$. Formation of adrenolutin was followed by the increase in optical density at 410 $m\mu$. The quantity of adrenolutin formed relative to adrenochrome lost was determined by dividing the optical density decrease at 480 by the increase in optical density at 410. High ratios indicate small conversion of adrenochrome into adrenolutin.

The ratios for schizophrenic and non-schizophrenic patients are shown in Table 4.

Thus in vitro schizophrenic plasma also converts more adrenochrome into adrenolutin. The ratio 0.91 differentiates the schizophrenic group from the other patients ($\chi^2 = 4.8$). Other patients and normals have similar distributions. Similar studies have been reported with alcoholic psychoses (50).

EFFECT OF LSD AND BOL-148 UPON ADRENOCHROME PLASMA LEVELS

Adrenochrome levels were determined after administration of LSD and BOL-148 orally to patients and volunteers. The results are shown in Table 5.

It is likely the LSD increases adrenochrome levels by somehow interfering with its destruction in the brain, as it does in plasma.

DISCUSSION

Adrenochrome is converted in plasma into adrenolutin (41) in vivo and in vitro and perhaps into a series of leuco substances.

TABLE 4

Showing Relative In Vitro Conversion of Adrenochrome to Adrenolutin. Ratio is the Decrease in Optical Density at 480 $m\mu$ Divided by Increase in Optical Density at 410

Group	N	Mean	Range	Number under .91	Number 0.91 and over
Schizophrenic	11	0.78	.36–1.12	10	1
Other Patients*	10	1.04	.54–1.40	3	7
Normals	10	1.25	.55–2.05	1	9

* Four are non-psychiatric cases undergoing surgery. Blood drawn during operation.

TABLE 5

Effect of LSD-25 and BOL-148 Upon Adrenochrome Stability In Vivo

Subject	Treatment	Dose (micrograms)	Adrenochrome, micrograms/liter plasma (hours)		
			0	½	1
Normal	LSD-25	25	52	—	81
Normal	LSD-25	35	17	—	115
Normal	LSD-25	35	52	166	—
Normal	LSD-25	35	52	—	81
Normal	LSD-25	100	47	—	125
Alcoholic	LSD-25	200	44	—	122
Normal	BOL-148	500	71	—	63
Normal	BOL-148	500	70	107	—
Mean	LSD-25		44		105

The evidence outlined suggests that adrenochrome is formed in the brain and is there and in blood converted into these other substances. The assumption may be made that schizophrenic patients have chemical substances which inhibit the conversion of adrenochrome to substances which are not psychotomimetic and thus increase the conversion into adrenolutin which is. With this assumption, it is possible to account for (1) higher levels of adrenochrome in cerebrospinal fluid where it is slowly destroyed (42) and normal levels in blood where it is rapidly destroyed, (2) a decreased tolerance for adrenochrome administered intravenously, and (3) the increased conversion of adrenochrome to adrenolutin in vivo and in vitro. It may be assumed that LSD inhibits the same reaction and this reproduces a schizophrenic-like reaction whereas BOL-148 does not.

The increase in the production of indole substances derived from epinephrine may account partially for the findings recently reviewed by McGeer (62, 63, 64) and Buscaino (9) for aromaturia in schizophrenia.

The possibility that schizophrenia fluids contain substances which interfere with metabolism of adrenochrome and thus perhaps of epinephrine is strongly supported by recent studies of Walaszek and Minz (68, 102). They found that the rabbit hypothalamus treated with schizophrenic serum compared with normal serum contained the following quantities of norepinephrine and epinephrine.

	Normal serum	Schizophrenic serum
Norepinephrine	3.3 μg/gram	7.0 μg/gram
Epinephrine	0.11 μg/gram	0.90 μg/gram

Schizophrenic serum apparently contains factors which interfere with the utilization of epinephrine.

Fellman's finding (21) that epinephrine may form some of the pigments of substantia nigra through oxidative polymerization, *i.e.*, through adrenochrome to melanin-like pigments supports the suggestion that adrenochrome plays a role in brain metabolism.

CONCLUSION

Adrenochrome metabolism is disturbed in schizophrenic and in normal subjects given LSD. The first crucial experiment receives substantial support. It is likely that the bodies of schizophrenics contain substances which inhibit the transformation of adrenochrome into leuco adrenochrome thereby increasing the relative production of adrenolutin.

ADRENOCHROME AND ADRENOLUTIN ARE PSYCHOTOMIMETIC SUBSTANCES

PSYCHOLOGICAL ACTIVITY OF ADRENOCHROME AND ADRENOLUTIN

These substances were first studied because of their chemical relationship to epinephrine and their structural similarity to other well known hallucinogens, *i.e.* LSD, an indole, and mescaline, potentially an indole. Evidence shows that mescaline may become indolized in the body (27).

In several instances, deteriorated epinephrine has produced psychological changes resembling schizophrenia. In one case, large quantities of adrenaline alone produced an LSD-like experience (42); in another pink epinephrine inhaled by an asthmatic for one month produced a change difficult to distinguish from an acute schizophrenic reaction (71). The symptoms continued some weeks after discontinuing the epinephrine. In another (100), a paranoid psychosis apparently followed inhalation of yellow epinephrine and in a fourth (91), every time brown epinephrine was taken, the patient experienced what he called a "crazy feeling."

Adrenochrome and adrenolutin have produced the following changes: (1) catatonia in cats when given by vein or into the ventricles of the brain (65, 78, 87), (2) catatonia in monkeys when given by vein (65), (3) EEG changes in cats when placed in the ventricles and in epileptic humans after intravenous injection (96).

Psychologically, these substances (adrenochrome subcutaneously, intravenously, sublingually (97) or as an inhalant (72); adrenolutin by mouth or vein (38)) have produced the following changes: (1) change in thought similar to those seen in schizophrenia; *i.e.* blocking, referential thinking, delusions, slowing of thought, lack of insight, decrease in level of abstraction, (2) depression often long sustained, often with a paranoid tinge, (3) changes in personality from pleasant mild mannered individuals to hostile difficult states, (4) changes in visual perception and rarely hallucinations, (5) decreases in anxiety and (6) marked increases in fatigue and irritability.

Changes usually occur within one hour after the compound has been administered. Thought processes are substantially normal within twelve hours. Mood and personality change may last as long as two weeks. Transient perceptual changes occur after the drug is administered. In one instance, acute schizophrenia followed the administration of 25 mg. of adrenochrome. This illness lasted six months (71).

Schizophrenic serum injected very rapidly into normal volunteers induces some schizophrenic-like changes (30). Taraxein extracted from schizophrenic serum (29, 55) produces remarkable catatonic like reactions and other schizophrenic changes in normals. Taraxein does not contain adrenochrome nor adrenolutin. It does however increase the excretion of indolic substances in urine when administered to normals (30).

The complete adrenochrome hypothesis (45) postulated two essential conditions for the development of schizophrenia: (1) an increase in the concentration of acetylcholine, (2) an increase in the concentration of adrenochrome and/or adrenolutin. LSD inhibits pseudo choline esterase and increases brain acetylcholine levels (99). There is a marked increase in adrenochrome levels after LSD is given (39, 40).

When ascorbic acid is given during the LSD experience in large dosages (1 gram every hour) the adrenochrome levels do not rise. The LSD experience is altered but not completely inhibited. Perceptual changes are not changed. There is less abnormality of thought and less paranoia. Depression is rare while euphoria is more common. When ascorbic acid is given for some days (5 grams per day for two days) before LSD, adrenochrome does not increase as much in plasma. The experience tends to be more pleasant, the patient is more cooperative and better able to concentrate (40).

Brom LSD (BOL) differs very little from LSD biochemically and physiologically but in half mg. dosages per human produces little psychological change (85). BOL produces no change in adrenochrome levels (40).

There is other evidence that the LSD

experience depends upon changes in epinephrine metabolism. After LSD or mescaline has been given, epinephrine production is increased (56, 59). Those enzymes which convert production to adrenochrome are increased (74), as shown by (a) an increased conversion of epinephrine to adrenochrome semicarbazone and (b) an increased conversion of adrenaline to adrenolutin. It has recently been found that when rats are given LSD there is a marked increase in the P^{32} uptake by the adrenal medulla and diaphragm but not in brain cortex (57). This increase indicates increased cellular activity.

These authors consider that this response is specific for LSD because while in general stress reactions the adrenal cortex also becomes more active, it did not do so with LSD.

The effect of LSD on phosphate metabolism (32, 33) is similar to that of adrenolutin (see following section).

These findings that (1) LSD increases epinephrine production, (2) LSD increases the concentration of enzyme which oxidize epinephrine, (3) LSD increases activity of adrenal medulla, (4) LSD interferes with phosphate metabolism, (5) LSD markedly increases adrenochrome levels, (6) BOL does not increase adrenochrome—all strongly suggest the model psychosis may be dependent upon the formation of adrenochrome. In cases where no adrenochrome is formed, there will be no reaction. This apparently contradicts our observation that there is no increase in adrenochrome levels when LSD is given to subjects in combination with ascorbic acid. The experience is modified but not eliminated. There are two possible explanations: that adrenochrome levels may be elevated centrally but the rate of destruction in plasma is sufficiently rapid to maintain levels normal, and that the increased turnover of adrenochrome even though levels remain normal will yield increased quantities of adrenolutin centrally or peripherally or both.

POTENTIATION OF ADRENOCHROME EXPERIENCE

Adrenochrome and adrenolutin produce a psychological change which is most easily demonstrated in thought and mood. No changes in perception typical of LSD are usually present. The very marked visual changes often found in LSD and mescaline model psychoses are sometimes seen in acute severe schizophrenia and have been reported in the literature. These grosser changes are more likely to be noticed.

We have a curious situation—because with adrenochrome, the minor visual changes may not be discussed by the subject except in misleading terms such as, "things look different," etc. The investigator may assume that nothing has happened. But when LSD and mescaline are given, the marked visual changes are readily dismissed as "toxic." If animals are pretreated with LSD the response to adrenolutin is much potentiated (65). In humans potentiation occurs if ten mg. adrenochrome and adrenolutin are given one and one half hours after 35 μg. of LSD. Some subjects who reacted slightly to 100 μg. or more of LSD have their experiences potentiated by giving 10 mg. adrenochrome.

CONCLUSION

The similarity of the experiences induced by adrenochrome and adrenolutin alone or after pretreatment with small quantities of LSD provides support for the second crucial experiment.

THE SIMILARITY BETWEEN EXPERIMENTAL PSYCHOSES AND SCHIZOPHRENIA

INTRODUCTION

In normal volunteers, LSD and mescaline produce psychological changes which have much in common with schizophrenia (70, 81, 82). Investigators working with these compounds and more recently with adrenochrome and adrenolutin have been challenged to prove that there is some similarity between these two experiences. Bleuler

(5) terms these experiences a toxic psychosis whereas Denber (14) suggests the experience is neither a toxic psychosis nor schizophrenia. While it is true that one might support either contention both neglect theoretical principles in the use of models.

Scientists often have to discover ways of producing natural phenomena in the laboratory where they can be studied at leisure. A model is not a reproduction of an original. If it were, it would not be a model. A model is required to clarify certain aspects of an original. This the psychotomimetic substances do reasonably well. There undoubtedly is more similarity between the mescaline experience and schizophrenia than between a simple deteriorated schizophrenic without hallucinations and delusions and a wildly excited catatonic schizophrenic with vivid visual and auditory hallucinations. There is less similarity between toxic confusional states and schizophrenia.

Toxic confusional states are characterized by the presence of one or more of the following: (1) disorientation, (2) some disturbance in consciousness, (3) and some memory difficulty. These are usually found in deleria produced by fever, bromide, etc. Even then it is sometimes difficult to be sure of the diagnosis which depends upon the presence of an infection (fever) or the knowledge that the patient has been taking some noxious substance. A rapid recovery when the patient no longer has access to the drug supports the inference.

As normally used, the psychotomimetic drugs do not produce the confusion or clouding found in toxic states. They therefore are "toxic" only because they have been produced by a chemical substance given by the investigator. Unless this semantic trap is recognized for what it is, we may one day be forced to call schizophrenia a toxic psychosis.

To compare a model psychosis with schizophrenia, the clinical and biochemical responses of normal subjects given the particular psychotomimetic substances under investigation are compared. If these substances really are models, the similarities should be more striking than the differences.

PSYCHOTOMIMETIC ANTAGONISTS AND SCHIZOPHRENIC THERAPEUTIC AGENTS

For many years, barbiturates have been an important weapon in treating schizophrenia. Intravenous amytal will often dramatically relieve a psychosis but unfortunately this is fleeting. Similarly amytal will remove the LSD psychosis but when the amytal is excreted it returns.

Chlorpromazine is considered helpful for many excited schizophrenics especially those in chronic mental hospitals and it too will remove most of the features of the psychotomimetic drugs (34, 88). The value of reserpine, which depletes brain of serotonin, norepinephrine, and epinephrine (10, 11), in treatment of schizophrenia is as disputed as is its capacity for antagonizing LSD. The effects of Frenquel in both schizophrenia and as an LSD antagonist are equally vague (15, 16). Succinic acid was originally used to abort the mescaline experience (86, 92) and this has led to its use in the treatment of schizophrenia (8).

When used in large doses (3 grams/day), nicotinic acid modified the LSD experience (1) and is also useful in the treatment of early schizophrenia (46). Using 0.5 gram nicotinamide, the LSD experience was not modified (66). Nicotinic acid removes the E.E.G. dysrhythmia produced by intravenous adrenochrome (96) and also terminates the adrenolutin experience in humans (42).

Anxiety which intensifies the schizophrenic panic also markedly intensifies the LSD experience. An experience which is pleasant and euphorient may easily be converted into a state of panic.

Thus, it may generally be said that both conditions are aggravated by similar drugs and similar situations.

PSYCHOTOMIMETIC POTENTIATORS

Atropine can produce psychotic reactions that are very difficult to distinguish from schizophrenia. Datura has long been used in both in India and Africa to intensify the hashish psychosis (51).

Forrer and Goldner (25) in a study of LSD pretreated some subjects with atropine. An examination of their protocols suggest there was some intensification. However, more recently it has been shown that atropine does not increase the intensity of the LSD experience (66).

Benzedrine derivatives either in very large single doses or when taken for prolonged periods produces a schizophrenic like condition. It reactivates the LSD-psychosis after the experience has waned (48).

AUTONOMIC CHANGES

In both schizophrenia and model psychoses, changes in blood pressure, pulse rate and other indices of autonomic activity occur (85). Mayer-Gross found that LSD tended to elevate blood sugar slightly (61). A proportion of schizophrenics have abnormal sugar tolerance tests (67). This has not yet been studied in model psychoses. Temperature changes in humans are not significant. In animals, where larger dosages are permissible, it has been found that adrenochrome and trihydroxy-N-methyl indole produce a marked lowering of body temperature (49), concomitant with an increase in oxygen consumption (hypothalamic activity) and that LSD has a pyretogenic action on rabbits (11).

LIVER FUNCTION (79)

These tests are notoriously difficult to carry out and to interpret. Quastel when using the excretion of hippuric acid as an index (77) reported that schizophrenics showed disturbances in liver function. Fischer *et al.* (24) confirmed his finding and observed that under the influence of mescaline there is a similar disturbance in liver function using the Snapper cinnamic acid test. Schizophrenics cannot combine glycine with sodium benzoate even though they excrete increased quantities of glycine in their urine. We have found that the concentration of glycine in schizophrenic urine is greater than in non schizophrenic urine even though the total volume of schizophrenic urine is greater. However, when these schizophrenics are given a larger quantity of atropine (3 mg.) there is a marked decrease in urinary free glycine, whereas in normals there is an increase (98). It appears that there is usually a higher level of glycine production in schizophrenics but when there is a high demand for it as by the admission of atropine or benzoate, this free glycine is quickly depleted.

CHANGES IN URINARY CONSTITUENTS

A double blind series of experiments were used to gauge the psychotomimetic effects of adrenolutin (36). Normal volunteers receiving one of four treatments, one week apart: (1) placebo—placebo, (2) placebo—adrenolutin, (3) adrenolutin—placebo, (4) adrenolutin—adrenolutin. Subjects all expected to receive two different active psychotomimetic compounds. In each case, urines were collected the day before each experiment and the day of the experiment as follows: (1) 4–6 P.M., (2) 6–8 P.M., (3) 8–10 P.M., (4) 10 P.M. to first morning specimen usually 7 A.M. Total specimens were collected in each case. The urines were analyzed for (1) total volume, (2) pH, (3) total phosphate, (4) sodium and potassium, (5) uric acid, and (6) creatinine. The series includes eleven schizophrenics, nineteen normals receiving adrenolutin, seventeen receiving placebo and thirty five controls.

1. *Volume.* The mean volumes excreted per minute at the collection periods are shown in Table 6.

TABLE 6

Urine Excretions, ml/minute

	4–6 P.M.	6–8 P.M.	8–10 P.M.	During Night
Schizophrenia	2.40	1.67	1.51	0.51
Adrenolutin—50 mg.	1.03	1.71	1.27	0.53
Adrenolutin—25 mg.	—	2.58	0.97	0.49
Placebo	1.47	1.90	1.70	0.58
Control	1.09	1.16	1.29	0.64
Mean	1.50	1.80	1.35	0.55

TABLE 7

pH of Urine

	6 P.M.	8 P.M.	10 P.M.	Night Urine
Schizophrenics	6.5	6.4	5.9	5.5
Adrenolutin—50 mg.	6.7	6.8	7.2	6.1
Adrenolutin—25 mg.	6.0	5.8	6.1	5.7
Placebo	6.6	6.8	7.0	6.0
Control	6.2	6.4	6.8	6.1

TABLE 8

Phosphate Excretion, mg/minute

	4–6 P.M.	6–8 P.M.	8–10 P.M.	Night Sample	A.M. Calc.
Schizophrenics	—	.83	.83	.52	.21
Adrenolutin—50 mg.	.81	.76	.61	.51	.41
Adrenolutin—25 mg.	—	.78	.68	.63	.58
Placebo	.79	.70	.70	.68	.66
Control	.82	.94	.84	.70	.56

The urine collections taken the day before the experiment are the control samples. Changes in urine flow in the placebo group are probably due to anxiety.

For the first hour after taking adrenolutin, the subjects were just as anxious as those receiving placebo. By 8 P.M., adrenolutin subjects were not anxious.

The data shows that between 4 and 6 P.M., the schizophrenics excreted nearly twice as much urine per minute as the three normal groups. The three normal groups did not differ much from each other. Between 6 and 8 P.M., the schizophrenic, adrenolutin and placebo groups excreted more urine than the control group. The first three groups did not differ from each other.

By 10 P.M., the placebo group still excreted more urine while the schizophrenic group remained high. The secretion by the adrenolutin group was normal.

Using the mean excretion values at 6, 8 and 10 as representative of the daytime excretion (14 hours) and the night rate as representative for the night (10 hours), it is possible to calculate a daily excretion of 1868 ml./day for the schizophrenics, 1375 ml./day for the controls, 1768 for the placebo group and 1426 for the subjects receiving adrenolutin.

2. *pH.* The acidity values for the schizophrenic group show a gradual increase and the night urine was most acid. In the other three groups acidity decreased in the evening and increased slightly during the night.

3. *Phosphate.* Phosphate excretion is shown in Table 8.

There was no significant variance at each time period for the various groups. However, schizophrenics and subjects given 50 mg. adrenolutin retained more during the night. The values found for the night sample are mean values of the entire sleeping period. Values at 10 P.M. are different. It is therefore possible to calculate the excretion per minute toward the end of the collection period using the 10 P.M. value as representative of the rate for the first part of this collection period and the mean value for the entire period. These are shown in the table. Calculated schizophrenic excretions for the last interval are much lower than for all other groups followed by the normals receiving the most adrenolutin.

In order to check the validity of this calculation, urines were collected between 7 and 10 A.M. for twenty-eight schizophrenic patients and thirteen mentally ill non-schizophrenic patients residing in the same hospital. The mean phosphate excretion mg. per minute was 0.32 for schizophrenics and 0.53 for the other group. This difference in means is statistically significant (k = 2.10, p = 0.03).

Using the 6 or 8 P.M. values or their

mean when both are available as a baseline, the following retention of phosphate was found:

Schizophrenics	37%
Adrenolutin—50 mg.	35%
Adrenolutin—25 mg.	19%
Placebo	9%
Control	20%

The excretion of phosphorus per unit of creatinine for the period 6 to 10 P.M. is compared with the night excretion.

The adrenolutin effect of phosphate excretion is quite different from any placebo (anxiety) effect and adrenolutinized subjects show changes in phosphate excretion very similar to those seen in schizophrenia.

4. *Sodium.* The control group showed a fifty per cent decrease in per minute excretion during the night. With placebo, adrenolutin and schizophrenia the decrease was greater, *i.e.* there was more sodium retention. Using a night excretion rate of 1.5 mg. per minute as a dividing value, frequency distributions were found as presented in Table 11.

There was no significant difference between distributions of control and placebo and schizophrenia. Between control and adrenolutin, there was a significant difference ($\chi^2 = 9.0$) and between control, placebo and schizophrenia on one hand and adrenolutin on the other, also a significant difference ($\chi^2 = 4.4$).

5. *Creatinine.* Creatinine excretion is shown in Table 12.

6. *Discussion.* Urine volume is apparently sensitive to the presence of anxiety. By 8 P.M. normal subjects given placebo and adrenolutin excreted more urine than the control or schizophrenic group. However by 10 P.M. adrenolutin reduced excretion levels to or below the control level. This is probably due to the decrease in anxiety produced by adrenolutin, whereas with placebo anxiety was still present. During day hours, the schizophrenics excreted more urine but night rate was normal.

TABLE 9

Phosphate Excretion, mg. per mg. Creatinine

	Mean	Night	Change %
Schizophrenic	0.90	0.82	−9
Adrenolutin—50 mg.	0.49	0.43	−12
Adrenolutin—25 mg.	0.59	0.56	−5
Placebo	0.47	0.60	+28
Control	0.59	0.64	+8

TABLE 10

Sodium Excretion, mg/minute

	4–6 P.M.	6–8 P.M.	8–10 P.M.	Night Sample
Schizophrenic	4.46	4.05	2.84	1.91
Adrenolutin—50 mg.	3.80	4.54	4.12	1.41
Adrenolutin—25 mg.	—	3.31	2.61	1.63
Placebo	5.00	5.21	4.33	1.64
Control	4.73	4.65	4.94	2.39

TABLE 11

Sodium Excretion During Night, mg. per minute

	1.5 and Under	Over 1.5
Schizophrenia	4	8
Adrenolutin	12	7
Placebo	6	8
Control	10	25

TABLE 12

Creatinine Excretion, mg. per minute

	4–6 P.M.	6–8 P.M.	8–10 P.M.	Night Sample	A.M. Calc.
Schizophrenia	1.24	1.12	1.00	0.77	0.54
Adrenolutin—50 mg.	1.40	1.44	1.34	1.23	1.12
Adrenolutin—25 mg.	—	1.22	1.24	1.06	0.84
Placebo	1.68	1.52	1.59	1.30	1.01
Control	1.69	1.69	1.56	1.13	0.70

Anxiety did not alter urine acidity from control values and the diurnal rhythm was similar. Schizophrenic urine became progressively more acid from 6 P.M. until the next morning. With 25 mg. adrenolutin acidity diurnal rhythm resembled the schizophrenic group but with 50 mg. the changes were like the normal group.

Phosphate retention was not affected by anxiety. However, retention of phosphate was greater for schizophrenics and for adrenolutin at 50 mg. level. The other adrenolutin group showed little displacement from controls. Comparing the evening rate and morning rate it is seen that the excretion of phosphate per unit of creatinine is quite different separating placebo and controls on one hand from the other three groups.

The retention of phosphate following administration of adrenolutin resembles similar retention induced by LSD in humans (33) and in animals (4). Similarly the calculated morning excretion rates for schizophrenics which are about forty per cent of normal for the calculated late night rates (see Table 8) and sixty per cent for the collection period 7 to 10 A.M. are similar to those reported by Hoagland *et al.* (1953).

Excretion of potassium and uric acid was similar for all groups. Creatinine excretion was lowest for schizophrenics.

7. *Conclusion.* Some substances which aggravate or produce schizophrenic-like reactions act similarly with model psychosis. Conversely antagonists to model psychosis tend to be therapeutic for schizophrenia.

Similar autonomic and liver function disturbances are found in both states. Finally, adrenolutin and LSD produces changes in urinary acidity and phosphate similar to those found in schizophrenia. The third crucial experiment thus receives support.

TREATMENT OF SCHIZOPHRENIA

Treatment according to the adrenochrome hypothesis should be directed toward (1) decreasing adrenaline production, (2) decreasing the conversion of adrenaline into adrenochrome, (3) preventing the toxic action of adrenochrome and adrenolutin, and (4) removing or destroying adrenochrome and adrenolutin.

REDUCTION IN PRODUCTION OF ADRENALINE

Barbiturates and tranquilizers may decrease the production of adrenaline by decreasing sensory input. Psychotherapy and other environmental therapy may similarly reduce adrenaline production by decreasing fear and anxiety. Chemical control is often easier.

Few substances are known which decrease the production of adrenaline. Adrenolytic agents do not decrease adrenaline output nor "lyse" circulating adrenaline but reduce the sensitivity of the receptors. They may even increase the amount of adrenaline and so make matters worse.

Nicotinic acid and its amide are methyl acceptors which when they are used in large dosages may compete with noradrenaline for methyl groups and so decrease the adrenaline output. This was one reason why nicotinic acid was tried for schizophrenia. The results for early schizophrenia are good (46). About fifteen per cent of our schizophrenic patients require daily administration of this vitamin and relapse a few weeks after stopping it. It does not help chronic schizophrenics even when such massive doses as 25 grams a day are given for three months.

DECREASED CONVERSION TO ADRENOCHROME

The oxidation of adrenaline to adrenochrome may be inhibited by decreasing the redox potential of blood. Large quantities of ascorbic acid, glutathione or cysteine will do this. Complete inhibition can be produced in vitro.

DECREASED PRODUCTION OF ADRENOLUTIN FROM ADRENOCHROME

Adrenochrome is partially converted by plasma into adrenolutin. In the presence of ascorbic acid, more is converted into leuko adrenochrome. Both substances are stable in plasma. Since leuko adrenochrome is not psychotomimetic (65) whereas adrenolutin is, the pathway of adrenochrome degradation may determine the response.

These large quantities of ascorbic acid will decrease the conversion of adrenaline to adrenochrome and will accelerate the conversion of adrenochrome into its non toxic derivative.

In a few preliminary trials, 10–20 grams ascorbic acid have been tried in acute schizophrenia with encouraging results but further controlled trials are needed.

REMOVAL OF ADRENOCHROME AND ADRENOLUTIN

So long as the adrenaline production remains high in blood and in brain, it is not enough to remove both or either of these substances from blood. However, the immediate response may be good. For instance if all the blood was removed and replaced much adrenochrome would be removed and the capacity to destroy adrenochrome released from brain increased. In fact, massive transfusions have been used (15) with some but usually transient success.

Ascorbic acid, glutathione and cysteine all transform adrenochrome into leuko adrenochrome and so might be therapeutic. Recently glutathione has been used with some success in treatment (94). Schizophrenics tend to have low levels of ascorbic acid (31) and glutathione (2) in the blood. This has usually been explained by blaming inadequate feeding but it might equally well be due to an increased release of adrenochrome into the blood.

It has recently been shown that ascorbic acid and nicotinic acid combined give partial protection against adrenolutin given to monkeys (65). Ceruloplasmin, the copper bearing enzyme, which oxidizes p-phenylenediamine and other amines also gives partial protection. This seems to be due to binding by ceruloplasmin. The evidence for this (1) in the presence of ceruloplasmin, adrenolutin does not dialyze which it does do when alone or with other globulins, (65) and (2) in micromolar concentrations, adrenolutin markedly inhibits the activity of ceruloplasmin (75).

Ceruloplasmin has been used as a treatment for schizophrenia with encouraging results (60). It is, however, difficult to obtain for the richest human source is human placenta. The highest concentration of ceruloplasmin are during the last three months of pregnancy. Two weeks after the delivery, the blood levels are normal. It may be significant that puerperal psychosis usually start shortly after delivery. Perhaps this sudden decrease in ceruloplasmin after parturition results in a small proportion of women being subjected to toxic quantities of adrenolutin. If this is so, a possible treatment for these psychoses would be a transfusion from a pregnant woman.

It is noteworthy that during pregnancy especially just before the onset of labor the levels of adrenaline rise. During labor there is a marked decrease (84). The increase in the production of adrenaline might account for the increase in the concentration of oxidative enzymes necessary to destroy the adrenaline. Thus ceruloplasmin levels might be expected to have a high correlation with general stress reactions (30). This may also account for the puzzling relationship between toxemia of pregnancy which is more frequent in schizophrenic women than in normals. Possibly both are related to inadequate amounts or quality of ceruloplasmin. The extremely uncertain effect of pregnancy on schizophrenia may also have some bearing there. While a rise in ceruloplasmin might be very beneficial to a schizophrenic, if this were insufficient a relapse might occur. Clearly much work is needed to unravel these matters.

A function of adrenochrome may be to give protection against histamine which may account for the scarcity of allergic states in schizophrenia (53) and their extraordinary tolerance of very large doses of histamine (58).

Any substance which binds adrenolutin may be therapeutic. Zinc, for instance, which has a specific attachment for adrenolutin and forms a chelate is always present in insulin. It may be that the effect of deep insulin (presently being questioned (7)) is due as much to its zinc content as anything else. Insulin would merely act as a zinc carrier. We are starting to explore the effect of zinc salts.

CONCLUSION

Model psychoses and schizophrenia have been compared using four different experimental methods which assume that there is a major biochemical abnormality in this illness. First, it is now possible to discriminate between schizophrenic groups and others on the basis of several biological and biochemical assays which indicate the presence of unusual biochemical substances in schizophrenic blood and urine. The adrenochrome hypothesis predicts that these differences ought to be present and may lead to the discovery of specific schizophrenic metabolites arising from adrenochrome.

Second, adrenochrome and adrenolutin produce psychological changes in normals which reproduce some of the findings in schizophrenia. Third, phosphate excretion is altered by both adrenolutin and LSD-25 in a way which resembles the excretion in schizophrenics and not that in anxiety. Substances which antagonize psychotomimetics tend to be therapeutic for schizophrenia whereas substances which potentiate are not. Finally, treatments based on the adrenochrome hypothesis have shown some therapeutic success for early schizophrenic patients.

The evidence presented shows that schizophrenia and the model psychosis have many more similarities than differences and that the adrenochrome hypothesis is a useful model for schizophrenia which may be useful in developing better models.

REFERENCES

1. Agnew, N. and Hoffer, A. Nicotinic acid modified lysergic acid diethylamide psychoses. J. Ment. Sci., **101**: 12–27, 1955.
2. Altschule, M. D., Henneman, D. and Goncz, R. Carbohydrate metabolism in brain disease. VII. The effect of glutathione on carbohydrate intermediary metabolism in schizophrenia and manic depressive psychoses. A.M.A. Arch. Int. Med., **99**: 22–27, 1957.
3. Baruk, H. and Camus, L. La toxicite de la bile des schizophrenes. Second International Congress of Psychiatry, Zurich, 1957.
4. Bergen, J. R. and Beisaw, N. E. LSD and urinary inorganic phosphate excretion. Am. Physiol. Soc., **15**: 15, 1956.
5. Bleuler, M. Psychiatrische inturner en der serotonin forschung. Deut. Med. Wochenschrift, **27**: 1078–1081, 1956.
6. Blough, D. S. Some effects of drugs on visual discrimination in the pigeon. Ann. New York Acad. Sciences, **66**: 733–739, 1957.
7. Bourne, H. The insulin myth. Lancet, II, 964, 1953.
8. Burrell, R. H. Succinate therapy in mental hospital practice. Unpublished thesis. University of New Zealand. May, 1956.
9. Buscaino, V. M. Pathogenese et etiologie biologiques de la schizophrenie. Acta. Neurol., **13**: 1–26, 1958.
10. Carlsson, A., Rosengren, E., Bertler, A. and Nilsson, J. Effect of reserpine on the metabolism of catechol amines. In *Psychotropic Drugs*, Garattini, S. and Ghetti, V., Eds. Elsevier Publication Co., New York, 1957.
11. Cerletti, A. Lysergic acid diethylamide and related compounds. Neuropharmacology. Josiah Macy Jr. Foundation, 1955.
12. De Jong, H. H. *Experimental Catatonia*. Williams & Wilkins Co., Baltimore, 1945.
13. De Jong, H. H. A historical and personal approach to the development of experimental psychiatry. J. Clin. & Exper. Psychopath. **17**: 388–404, 1956.
14. Denber, H. Discussion. Soc. Biol. Psychiatry, Chicago, Ill., 1955.
15. Fabing, H. D. Biochemistry and schizophrenia. Royal Society Med., London, 1957.
16. Fabing, H. D. New blocking agent against the development of LSD-25 psychosis. Science, **121**: 208, 1955.
17. Fabing, H. D., Kropa, E. L., Hawkins, J. R. and Leake, C. D. Bufotenine effects in humans. Fed. Proc., **15**: 421, 1956.
18. Fedoroff, S. Toxicity of schizophrenic blood serum in tissue cultures. J. Lab. Clin. Invest., **48**: 55–62, 1956.
19. Fedoroff, S. Influence of blood serum of schizophrenics in cells in tissue culture. Ph.D. Thesis, University of Saskatchewan, 1958.
20. Fedoroff, S. and Hoffer, A. Toxicity of blood serum from schizophrenic and non-schizophrenic subjects. J. Nerv. Ment. Dis., **124**: 396–398, 1956.
21. Fellman, J. H. Epinephrine metabolites and pigmentation in the central nervous system in a case of phenylpyruvic oligophrenia. J. Neurol. Neurosurg. & Psychiat., **21**: 58–62, 1958.
22. Fischer, P. and Lecomte, J. Metabolisme de l'adrenochrome de ses produits de reduction et du tri hydroxy-N-methyl indol ches le lapin, le chat, et la chien. Bull. Soc. Chem. Biol., **33**: 569–600, 1951.
23. Fischer, P. and de Landtsheer, L. The metabolism of adrenochrome and trihydroxy-

methylindole in the rabbit. Experentia, **6:** 305–306, 1950.

24. Fischer, R., Georgi, F., Weber, R. and Piaget, R. M. Psychophysische Korrelationen. Schweiz. med. Woch., **80:** 129, 1950.

25. Forrer, G. R. and Goldner, R. D. Experimental physiological studies with lysergic acid diethylamide. A.M.A. Arch. Neur. Psychiat., **65:** 581–588, 1951.

26. Gershenovich, Z. S. Adrenaline substance of the brain and suprarenals under increased oxygen pressure. Ukraine Biokhim. Zhur., **27:** 3, 1955.

27. Harley-Mason, J., Laird, A. M. and Smythies, J. R. The metabolism of mescalin in the human. Second Int. Congress of Psychiatry, Zurich, 1957.

28. Heacock, R., Nerenberg, C. and Payza, N. The chemistry of the aminochromes. Part I. The preparation and paper chromatography of pure adrenochrome. Canad. J. Chem., **36:** 853–857, 1958.

29. Heath, R. G., Martens, S., Leach, B. E., Cohen, M. and Angel, C. Effect on behavior in humans with the administration of taraxein. Am. J. Psychiatry, **114:** 14–24, 1957.

30. Heath, R. G., Leach, B. E., Byers, L. W., Martens, S. and Feigley, C. A. Pharmacological and biological psychopathology. Amer. J. Psychiatry, **114:** 683–689, 1958.

31. Heath, R. G. and Leach, B. E. *Research in Schizophrenia: Changing concepts in psychoanalytic medicine*. Grune and Stratton, New York, 1956.

32. Hoagland, H. A review of biochemical changes induced in vivo by lysergic acid diethylamide and similar drugs. Ann. New York Acad. Sci., **66:** 445–458, 1957.

33. Hoagland, H., Rinkel, M. and Hyde, R. W. Adrenocortical function and urinary phosphate excretion. Arch. Neur. Psychiat., **73:** 100–109, 1955.

34. Hoch, P. Studies in routes of administration and counter acting drugs. *Lysergic acid diethylamide and mescaline in experimental psychiatry*, Cholden, L., Ed. Grune and Stratton, New York, 1956.

35. Hoffer, A. Adrenolutin as a psychotomimetic agent. In *Tranquilizing Drugs*, Himwich, H. E., Ed. Amer. Assoc. Advancement Science, Washington, D.C., Publ. 46, 1957.

36. Hoffer, A. Adrenolutin as a psychotomimetic agent. In *Hormones, Brain Function and Behavior*, Hoagland, H., Ed. Academic Press, New York, 1957.

37. Hoffer, A. Adrenochrome and adrenolutin and their relationship to mental disease. In *Psychotropic Drugs*, Garattini, S. and Ghetti, V., Eds. Elsevier Publication Co., New York, 1957.

38. Hoffer, A. Epinephrine derivatives as potential schizophrenic factors. J. Clin. Exp. Psychopath., **18:** 27-60, 1957.

39. Hoffer, A. Adrenochrome in blood plasma. Am. J. Psychiatry, **114:** 752–753, 1958.

40. Hoffer, A. and Wojcicki, H. Unpublished observations. 1957.

41. Hoffer, A. *Mode of Actions of Ergot Hallucinogens*. Brain Research Foundation, New York, 1958.

42. Hoffer, A. Unpublished observation. 1957.

43. Hoffer, A., Osmond, H. and Smythies, J. Schizophrenia: a new approach. J. Ment. Sci., **100:** 29–45, 1954.

44. Hoffer, A. and Kenyon, M. Conversion of adrenaline to adrenolutin in human blood serum. Arch. Neur. Psychiat., **77:** 437–438, 1957.

45. Hoffer, A. and Osmond, H. Schizophrenia: an autonomic disease. J. Nerv. Ment. Dis., **122:** 448–452, 1955.

46. Hoffer, A., Osmond, H., Callbeck, M. J. and Kahan, I. Treatment of schizophrenia with nicotinic acid and nicotinamide. J. Clin. Exp. Psychopath., **18:** 131–158, 1957.

47. Hofman, A., Heim, R., Brach, A. and Kobel, H. Psilocybin ein psychotroper wirkstoff aus dem mexikanischen rauschpilz Psilocybe mesicana heim. Experentia, **14:** 107–109, 1958.

48. Hubbard, A. Personal Communication. 1957.

49. Hutcheon, D. E., Lowenthal, J. and Eade, N. Adrenochrome hypothermia in normal and adrenalectomized rats. Acta. Pharmacodyn., **106:** 90–99, 1956.

50. Jantz, H. Uber stoffwecheseluntersughungen bei alkoolpsychosen. Zentrabl. Ger. Neur. u. Psychiat., **137:** 141, 1956.

51. Johnson, D. M. *The Hallucinogenic Drugs*. Christopher Johnson, London, 1953.

52. Laidlaw, P. P. The physiological actions of indole ethylamine. Biochem. J., **6:** 141–150, 1912.

53. Lea, A. J. Adrenochrome as the cause of schizophrenia: Investigation of some deductions from this hypothesis. J. Ment. Sci., **101:** 538, 1955.

54. Lettre, R. Drug action on cells in vitro. Ann. New York Acad. Sci., **58:** 1085, 1954.

55. Lief, H. I. The effects of taraxein on a patient in analysis. Arch. Neur. Psychiat., **78:** 624–627, 1957.

56. Liddell, D. W. and Weil-Malherbe, H. The effects of methedrine and of lysergic acid diethylamide on mental processes and on the blood adrenaline level. J. Neur. Neurosurg. & Psychiat., **16:** 7, 1953.

57. Lingjaerde, P. and Skaug, D. E. A study of the uptake of radioactive phosphorus in the brain, endocrine and other organs of LSD-treated rats. J. Nerv. Ment. Dis., **124:** 578–584, 1956.

58. Lucy, J. Histamine tolerance in schizophrenia. Arch. Neur. Psychiatry, **71:** 629–639, 1954.

59. Manger, W. M., Schwarz, B. E., Baars, C. W., Wakim, K. G., Bollman, J. L., Petersen, M. C. and Berkson, J. Epinephrine and arte-

renal in mental disease. A.M.A. Arch. Neur. Psychiatry., **78:** 396–412, 1957.

60. Martens, S., Vallbo, S. and Melander, B. Administration of ceruloplasmin to schizophrenics. Meeting, Soc. Biol. Psychiatry, San Francisco. May, 1958.
61. Mayer-Gross, W., McAdam, W. and Walker, J. W. Psychological and biochemical effects of lysergic acid diethylamide. Nature, **168:** 827–828, 1951.
62. McGeer, E. G., Brown, W. T. and McGeer, P. L. Aromatic metabolism in schizophrenia. J. Nerv. Ment. Dis., **125:** 176–180, 1957.
63. McGeer, P. L., McNair, F. E., McGeer, E. G. and Gibson, W. C. Aromatic metabolism in schizophrenia. J. Nerv. Ment. Dis., **125:** 166–176, 1957.
64. McGeer, P. L. and McGeer, E. G. Aromatic metabolism in schizophrenia. Conference on Biochemistry of Mental Illness, U.B.C. June, 1957.
65. Melander, B. Personal Communication, 1957.
66. Miller, A. I., Williams, H. L. and Murphree, H. B. Niacin, niacinamide or atropine versus LSD-25 model psychoses in human volunteers. Fed. Proc., **16:** 169, 1957.
67. Meduna, L. J. and McColloch, W. S. The modern concept of schizophrenia. Med. Clin. N. America, **29:** 147–164, 1945.
68. Minz, B. and Walaszek, E. J. Tropic action of epinephrine on the cerebral cortex of rabbits pretreated with serum from schizophrenics. J. Pharm. Exper. Ther., **122:** 53A, 1958.
69. Osmond, H. Ololuiqui: the ancient Aztec narcotic. J. Ment. Sci., **101:** 526–537, 1955.
70. Osmond, H. A review of the clinical effects of psychotomimetic agents. Ann. New York Acad. Sciences, **66:** 418–434, 1957.
71. Osmond, H. Research on Schizophrenia. Neuropharmacology. Josiah Macy Jr. Foundation, New York, 1955.
72. Osmond, H. and Hoffer, A. The case of Mr. Kovish. J. Ment. Sci., **104:** 302–325, 1958.
73. Payza, N. and Mahon, M. The estimation of adrenochrome in human plasma. Personal Communication, 1958.
74. Payza, N. and Hoffer, A. Adrenaline oxidation by human plasma. 1958. (To be published).
75. Payza, N. Personal Communication, 1957.
76. Peretz, D. I., Smythies, J. R. and Gibson, W. C. A new hallucinogen: 3,4,5 trimethoxyphenylamino propane. J. Ment. Sci., **101:** 317–329, 1955.
77. Quastel, J. H. and Wales, W. T. Lancet, **I**, 402, 1940.
78. Rice, W. B. and McColl, J. D. Effect of intracerebral injection of hallucinogenic compounds. Proc. Can. Physiol. Soc., p. 47, 1957.
79. Richter, D. *Schizophrenia: Somatic Aspects.* Macmillan Co., New York, 1957.
80. Rieder, H. P. Biologische toxizitatsbestimmung pathologischer Korperflussigkeiten. Psychiatrie et Neurologia, **134:** 378–395, 1957.
81. Rinkel, M., Deshon, H. J., Hyde, R. W. and Solomon, H. C. Experimental schizophrenia-like symptoms. Am. J. Psychiatry, **108:** 572–578, 1952.
82. Rinkel, M., Hyde, R. W., Solomon, H. C. and Hoagland, H. Experimental psychiatry. II. Clinical and physiochemical observations in experimental psychoses. Am. J. Psychiatry, **111:** 881–895, 1955.
83. Rinkel, M., Hyde, R. W. and Solomon, H. C. Experimental psychiatry, IV. Dis. Nerv. System, **16:** 229, 1955.
84. Ritzel, G., Staub, H. and Hunzinger, W. A. Adrenaline during labour. German Med. Month., **2:** 110–111, 1957.
85. Rothlin, E. Pharmacology of lysergic acid diethylamide and some of its related compounds. In *Psychotropic Drugs*, Garattini, S. and Ghetti, V., Eds. Elsevier Publication Co., New York, 1957.
86. Schueler, F. W. The effect of succinate in mescaline hallucinations. J. Lab. Clin. Med., **33:** 1297–1303, 1948.
87. Schwarz, B. E., Wakim, K. G., Bickford, R. G. and Lichtenheld, F. R. Behavioral and electroencephalographic effects of hallucinogenic drugs. Arch. Neur. Psychiat., **75:** 83–90, 1956.
88. Schwarz, B. E., Bickford, R. G. and Rome, H. P. Reversibility of induced psychosis with chlorpromazine. Proc. Staff Meet., Mayo Clinic, **30:** 407–417, 1955.
89. Schwarzenbach, F. Second International Congress of Psychiatry, Zurich, 1957.
90. Schwarzenbach, F. Personal Communication, 1957.
91. Seddon, T. Personal Communication, 1957.
92. Stevenson, I. and Sanchez, A. The antidotal action of sodium succinate in the mescaline psychosis. Am. J. Psychiat., **114:** 328–332, 1957.
93. Stockings, G. T. A clinical study of the mescaline psychosis with special reference to the mechanism of the genesis of schizophrenic and other psychotic states. J. Ment. Sci., **86:** 29, 1940.
94. Surikov, M. P., Ushakov, G. K., Verbliunskaia, A. A. and Khokhlov, L. K. Glutathione in the treatment of mental patients. Zh. Nevropat., **57:** 237–240, 1940.
95. Szara, S. The comparison of the psychiatric effect of tryptamine derivatives with the effects of mescaline and LSD-25 in self experiments. In *Psychotropic Drugs*, Garattini, S. and Ghetti, V., Eds. Elsevier Publication Co., New York, 1957.
96. Szatmari, A., Hoffer, A. and Schneider, R. The effect of adrenochrome and niacin on electroencephalogram of epileptics. Am. J. Psychiat., **111:** 603–616, 1955.
97. Taubmann, von G. and Jantz, H. Untersu-

chung uber die dem adrenochrom zugeschenebenen psychotoxischen wirkungen. Der Nervenarzt., **20:** 485–488, 1957.

98. Trew, A. and Fischer, R. Faulty detoxication in schizophrenia. Lancet, **19:** 402, 1955.
99. Thompson, R. H. S., Tickner, A. and Webster, R. The action of lysergic acid diethylamide on mammalian cholinesterases. Brit. J. Pharm., **10:** 61–65, 1955.
100. Tomasson, H. Personal Communication. 1957.
101. Vogt, M. The concentration of sympathin in different parts of the central nervous system under normal conditions and after the administration of drugs. J. Physiol., **123:** 451–481, 1954.
102. Walaszek, E. J. and Minz, B. Neurohormonal changes in brains of rabbits pretreated with serum from schizophrenics. J. Pharmacol. Exp. Ther., **122:** 80A, 1958.
103. Winter, C. A. and Flataker, L. Effects of lysergic acid diethylamide upon performance of trained rats. Proc. Soc. Exp. Biol. Med., **92:** 285–289, 1956.
104. Winter, C. Effects of blood plasma and extracts of urine from psychotic patients upon the performance of trained rats. Conference on Biochemistry and Mental Illness. U.B.C., June, 1957.
105. Winter, C. Personal Communication. 1958.
106. Witt, P. N. Ein biologischer nachweis von adrenochrom und sein mogliche anwendung. Helv. Physiol. Acta., **12:** 327, 1954.

SURVEY OF VARIOUS ASPECTS OF TRANSVESTISM IN THE LIGHT OF OUR PRESENT KNOWLEDGE

N. LUKIANOWICZ, M.D., D.P.M.[1]

INTRODUCTION

PRELIMINARY REMARKS

The aim of this paper is to give a brief survey of various aspects of transvestism in the light of our present knowledge of this phenomenon, with a particular consideration of its etiology and symptomatology, and its cultural, social and legal implications. Very little may yet be said about therapeutic possibilities.

Transvestism is known, under various names, in almost all cultures and in all parts of the world. Its ubiquity led Ellis (29) to the conclusion that it "may possibly represent not ... a corrupt or overrefined manifestation of late cultures, but the survival of an ancient and natural tendency of more primitive man." If so, such cross-dressing might have had in the earlier cultures some magical, ritualistic or symbolic meaning, now entirely forgotten.

The phenomenon was already known in antiquity. It was described by Herodotes as the mysterious "Skythian illness" on the northern shores of the Black Sea. There, hitherto apparently normal men would put on female clothes, do women's work, and generally show feminine character and behavior (64). Its existence in classical Greece is suggested by the picture of Hercules dressed in female clothes and serving his mistress Omphale (30). Transvestism was also known in ancient Rome, particularly at the time of her decline, and some of the Roman Emperors (*e.g.* Caligula, Heliogabal) occasionally dressed in female garments (27, 96). At the beginning of modern times the most notorious transvestites were the three Frenchmen: the brother of King Henri III; the Abbe de Choisy; and the proverbial Chevalier d'Eon (24).

In our own epoch and culture the ever growing number of papers on transvestism suggests either an increase in the frequency of occurrence of this sexual aberration, or a greater interest in it on the part of writers. Thus, Masson (70), in her excellent survey of transvestism (in history, literature and ethnography), covering almost a century (1838–1935), found 69 cases of transvestism described by 29 authors; in two decades (1935–1955) following her study, 76 new observations were reported by 40 writers. (Our data have been based only on British, American, French, German, Danish and Swiss literature.)

TERMINOLOGY

The nomenclature of transvestism has undergone a real evolution. In 1876 Westphal (99) called this phenomenon "the contrary sexual feeling;" Krafft-Ebing named it "metamorphosis sexualis paranoica" (64); Carpenter coined the term "cross-dressing;" Ellis used in 1913 the clumsy name "sexo-esthetic inversion," which he changed to a more euphonic "Eonism" in 1920; Bloch (15) called transvestism "psychical hermaphroditism;" Tennenbaum (93) used the term "sexual inversion."

In 1910 Hirschfeld (52) introduced the term "transvestism," being fully aware "that this name indicates only the most obvious aspect of this phenomenon." His term became generally accepted, although it is still being used without the necessary discrimination between the two akin conditions, the real transvestism and transsexualism.

---

[1] Bristol Mental Hospitals, Barrow Hospital, Bristol, England.

DEFINITION

There is no generally accepted definition of transvestism as yet. For example: Pettow (84) defined it as "a psychological compulsion;" Ellis "as really a modification of normal heterosexuality;" Stekel (92) as "a mask for homosexuality;" Mayer-Gross, Slater and Roth (71) as "a form of fetishism in the homosexually inclined." In "Janett Thompson's" (96) view transvestism "falls into the category of a behavior problem rather than onto that of a sexual problem as it has usually been classified." Podolsky (86) defines it as "a form of compulsion neurosis," but a bit further says that "transvestism is a mental ailment." Kinsey (61) sees in a transvestite "an individual who prefers to wear the clothes of the opposite sex, and who desires to be accepted in the social organization as an individual of the opposite sex." Hamburger (49, 50) offers an almost identical definition. Benjamin (10, 11) regards transvestism as "a disharmony of the total sexual sense, a sexual indecision or a dissociation of the physical and mental duality".

We use the term "transvestism" for a sexual deviation characterized by: (a) a desire to wear the dress of the opposite sex, and (b) (less often) a wish to be looked upon, and to be socially accepted, as a member of this sex. The name "transsexualism" is applied to these rare cases of transvestism, where, apart from both just mentioned tendencies, there exists: (c) a persistent morbid urge to undergo a "conversion-operation," *i.e.* to have an anatomical "change" of the inborn sex.

CLASSIFICATION

Hirschfeld uses several different criteria in his classification of transvestism. Hence, although apparently elaborate and comprehensive, his division is complex, and is not homogeneous: 1, "*complete*" transvestite, desiring to have his sex "changed" (corresponding to our "transsexualist"); 2, "*partial*" transvestite, with the tendency to cross-dress only (identical with our "transvestite"); 3, *constant*, 4, *periodical* transvestite; 5, *transvestite in name* (adopting a given name belonging to the opposite sex); 6, *narcissistic*, 7, *homosexual*, 8, *bisexual*, 9, "*metatropic*" transvestite, with an inverted love object (*i.e.*, a male transvestite seeking the love of a mannish woman), and 10, "*automonosexual*" (autoerotic) transvestite.

Some authors base their classification on accidental and irrelevant factors. Thus Pettow divides transvestites into three groups, according to their sex and age: 1, men adopting women's garb; 2, women adopting men's garb, and: 3, adults adopting the garb of children. Kinsey takes as a criterion for his classification the factor of time only, thus recognizing: 1, "permanent transvestites," who "try to identify with the opposite sex... at all times," and 2, "partial transvestites, who adopt their changed roles only on occasion, as at home in the evening."

Other writers take as a principle of their classification the direction of the sexual drive. Thus Moll (74, 75) divides transvestism into: 1, hetero-, 2, homo-sexual, 3, non-psycho-sexual, obsessive type, and 4, a type in which the cross-dressing is due to some ulterior motive ("pseudo-transvestism"). Dukor (27) recognizes three groups of transvestites: 1, hetero-, 2, auto-, and 3, homo-sexual. Bättig (8) classifies transvestites into homo-, bi-, hetero-, and autosexual, according to their sexual expression.

Ellis divides transvestism into two basic groups: "One... in which the inversion is mainly confined to the sphere of clothing, and another... in which... the subject feels really to belong to" the opposite "sex, although he has no delusion regarding his anatomical conformation." Bättig also recognizes two types of transvestism: 1, an apparent ("unechter") type, "psychoreactive in origin," and 2, genuine ("echter") transvestism, comparatively rare, based on "a constitutional abnormality of an unequal foundation ("Anlage") of physique, drives and psyche," always connected with a strong desire for the "change" of the inborn sex. Similarly Hamburger makes a distinction between: 1, transvestism as a symptom, that may accompany some other sexual deviation, and 2, "genuine tranevestism, or psychic hermaphroditism," denoting "persons with the fundamental feeling... of the female personality in a male body."

Benjamin classifies transvestism into three types. The first class is called the "Principally psychogenic transvestite," in whom "the female component... is sufficient to allow an early psychological conditioning to form the transvestitic pattern in later life." This type of transvestite does not want to have his sex "changed." He wants the society to change its attitude and its restrictive laws towards him. The second, "The intermediate type", more active than the first, "inclines at times towards homosexualism." The third type is "The somatopsychic transsexual," where "the conviction of these endocrine males that they are really females with faulty sex organs is profound and passionate."

This paper accepts Benjamin's basic classification; the further differentiation of transvestism into certain clinical subgroups will follow in the section on symptomatology.

ETIOLOGY OF TRANSVESTISM

THEORY OF DIVINE ORIGIN

Among various etiological hypotheses, the oldest attempt at explanation of transvestism has its roots in myth and magic: Herodotus explained the "Skythian illness" as a divine punishment, a curse, thrown upon the Skyths and their male posterity by the goddess Aphrodite, enraged with their plundering of her temple in Askalos.

THEORIES OF ORGANIC ORIGIN

Traumatic theory. Hippocrates assumed that transvestism among Skyths was caused by a repeated mechanical trauma to their reproductive organs, incurred by their excessive horse riding. This concept of traumatic origin of transvestism was revived at the beginning of the last century, when the impotence and effeminacy found among some Tartars was attributed to the same factor (Klaproth, 62).

Belief in the "demasculinizing" effect of horse riding found a practical application in a different part of the world: the Pueblo Indians forced some chosen men to excessive riding and exuberant masturbation, which supposed to have led to atrophy of their testicles and penes, and thus to a general femininization; in this way these Indians procured their "mujerados", a type of effeminate male transvestites, dressed in female garments, and used for ritual homosexual orgies during their religious spring festivities (Hammond, 51).

Genetic and endocrine theory. This theory is based on the assumption that the chromosomal, or "genetic," sex (which is contained in the nuclear structure of all body cells, and has been detected and demonstrated in the epidermal nuclei of the skin), does not always correspond to the respective gonadal, or "endocrine," sex. It means that an "endocrine" male may be a "genetic" female (and *vice versa*), with the consequent inverted psychosexual make-up. (Lang (65) believed that the male homosexuals were in fact "genetic females," whose body has undergone a complete "sex reversal" in the direction of maleness.)

The writers belonging to this school of thought regard transvestism as a kind of "intersexualtiy," (for instance Moszkowicz (78), Binder (13) and Hamburger) mainly basing their views on Goldschmidt's (43) findings in the gipsy moth and by analogy postulating the existence of human "Umwändlungsmänner" (*i.e.* males "undergoing change"). Similarly Dukor assumes in transvestism a "constitutional intersexuality," which may be due either to the basic poor differentiation of the sexual drive, or due to the "lack of equalization of the chromosome content of the hormonal balance." Benjamin also regards transvestism as a kind of "intersexuality." "There may be more or less pronounced irregularity in genetic and endocrine development with resultant "intersexes" of varying character, degree and intensity." Accordingly the male transvestite is expected to possess the female double X (*i.e.* "XX") sex-chromosome-complex in his body cells, thus presenting an individual with a male physique, but with female genetic and psycho-sexual constitution. Similarly Rosanoff (**88**) hypothesizes that "factors for psycho-sexual make-up, though they may be more or less linked to the factors for physical sexuality, must be to some extent independently transmitted." (If this were so, it might well account for various forms of discrepancy between the actual anatomical sex and the psychosexual drive.)

All these interesting biological considerations have recently received a serious setback by the findings of Moore and Barr (**76**), Moore, Graham and Barr (**77**) and Barr and Hobbs (**7**), that "the male transvestite bears the male XY sex-chromosome complex," and also that "all male pseudohermaphrodites have typical male-type epidermal nuclei" (Barr and Hobbs).

Cerebral theory. G. N. Thompson (**95**) postulates the existence of a hypothetical cerebral center, discharging "without normal cortical control," and suggests that the sexual anomalies might be a kind of release phenomenon, similar to Parkinsonism and Athetosis. A quotation from Gutheil (**47, 48**) is relevant here: "We know that clinical and laboratory data regarding these disorders are inconclusive and that, so far, no evidence has been procured to support a biologic concept of transvestism." Yet, he adds conciliatorily, "As for myself, I am ready to concede that some, as yet undiagnosed, consitutional conditions may play a part in the etiology as predisposing factors."

THEORY OF PSYCHOLOGICAL CONDITIONING

This theory blames an adverse psychological conditioning in childhood for the development of transvestism (*cf.* Mead, **72, 73**; Bender and Paster, **9**; "Janett Thompson", and many others). Here is a brief survey of some of these traumatizing factors:

Parental rejection of a child because of his "unwanted" sex leads to feelings of inferiority and insecurity, to an unhealthy precocious preoccupation with problems of femininity and masculinity, to a confusion regarding his own sexual identity, and finally to transvestism. For example: **1.** Gutheil's **34** year old female transvestite, rejected by her mother, was well aware of the reason of her rejection: "Had I been a boy, everything would have been different." She lived with a **64** year old woman (a substitute for her real mother), whom she called "mother." **2.** The female transvestite of Barahal (**6**) was rejected by her mother because of her sex. Her cross-dressing was a symbolic appeal to her mother: "Love me. I am a boy, just as you always wanted me to be." **3.** Our patient "A" (**68**) also endeavored to buy his father's acceptance and love by pretending to be "a little girl," *i.e.*, cross-dressing.

Further, being rejected by their parents for their sex (*i.e.* their "unwanted" genitals), some patients develop a hostile, sadomasochistic attitude towards their own genitalia. They direct their resentment and their hatred to this visible "cause" of their rejection and unhappiness, and want to have them: 1, either hidden under the dress of the sex to which their parents wanted them to belong, and which they try by their cross-dressing to represent (transvestites);

or 2, completely removed (and possibly supplanted by sexual organs of the opposite sex) by means of a "conversion-operation" (*transsexualists*). In this way both types endeavor to acquire the acceptance and love of their rejecting parents.

Examples: Our patient "A" wrote, "I hated the fact that I had male organs". All patients of Worden and Marsh (103) "had a rejecting attitude toward their own genitals, regarding them as objects of contempt and ugliness." One of them described his feelings on ablation of his penis thus, "It is like having a cancer removed." Several of them "said that they masturbated to destroy the genital." Some patients make serious attempts at self-mutilation (*e.g.* the patients of Wyrsch (104), of Grotjahn (45) and of Bätting). Such attempts at the destruction of the hated sexual organs betray feelings of guilt and introjected hostility with self-destructory tendencies, identical with those met in depressive suicidal patients. The difference is only quantitative; in depression the patient wants to destroy his life, *i.e.* his whole body, in transsexualism only his genitalia. Yet this quantitative difference would cease to exist if Fenichel's (31) equation "body = phallus" would be accepted as an explanation.

Dressing of children into the garb appropriate to the opposite sex is the next frequent etiological factor. Almost every transvestite alleges that he was dressed into "girl's dress," till he was three or four, or more years old. This may perplex him and prevent from identification with members of his inborn sex. Examples of this mechanism: The patient of James (56) and our patients "A" and "B" were all dressed in girls' clothes until they were four. Guiraud's (46) patient was dressed as a girl until he was nine.

Cross-dressing for "punishment" is another possible precipitator of transvestite tendencies by way of traumatic fixation. This only applies to male subjects, as only the female dress for men has a humiliating connotation in our culture. There follow some illustrations: London's (67) patient was "punished" by his mother by being forced to wear girl's clothes at the age of six. At first he felt "so shy and humiliated that when visitors came he would hide under the table." However soon he developed an unhealthy interest in the garments of his mother, and would put them on "whenever he had the opportunity" of doing so. Our patient "B" was "punished" in exactly the same way. Subsequently he developed a desire to wear permanently girl's clothes. Also Karpman's (58) patient, when a boy, was similarly "punished" by his mother. This soon brought about transvestite tendencies in him.

The favored status of "a little girl" in the family group played a causative role in many transvestite cases. The symbolism of transvestism here is self-explanatory: through assuming the dress of the preferred sex (and thus pretending to belong to it), the respective children want to participate in the preferential treatment of this sex. Illustrative examples: 1. The older sister of our patient "A" was favored by both parents, and recommended as an example to her brother, who soon started to imitate her in her clothes and her manners. 2. The preferential treatment of girls in the orphanage was a precipitating factor in the feminine identification and transvestism of our patient "B". 3. Frank, a six year old boy, "began to dress in female garb, disliked boys and played only with girls. He complained that his mother liked his sister more than himself," and "said he would rather be a girl" (Bender and Paster).

Close visual contact with a female (usually the mother or a sister) may lead to "a state of primary identification" with her (Greenacre, 44). An exhibitionistic exposure of this female figure is frequent, though rarely even suspected. As illustrations: 1. There are strong indications that the mothers of three little boys studied by Friend and his colleagues (39), exposed themselves in front of their sons. This led, on the one hand, to transvestite tendencies in these boys, on the other hand to caricaturizing the phallic female in their play activities. 2. Billy, a six year old boy (of Bender and Paster's group of children), "expressed a desire to have breasts like his mother and to urinate like her. He wanted to be a girl and have his penis taken off." Without assuming some exhibitionistic activity on the part of his mother it would be impossible to account for his knowledge of anatomy of female sexual organs. 3. Maternal exhibitionistic behavior was also evident in some other boys studied by Bender and Paster. For instance, Aberdine boasted that "while sleeping in his foster-parents' bedroom, he often explored his mother's breasts and genitalia." Soon he developed transvestite and homosexual trends: he showed a preference to dress as a girl, played only with girls and dolls, imitated actresses, his gestures and voice were deliberately effeminate and he wanted to be a girl. He became fond of a boy and called him "my husband". 4. Stanley, aged ten (of the same series), openly "admitted that his mother stimulated him sexually" (9). Often a similar exhibitionistic role may be played by "a sister, relatively close in age" (Greenacre), as it occurred in our patient "A", and in the patients of Fenichel, of Berman (12), and of Karpman.

A reversal of parental roles (*i.e.* an aggressive mother and a submissive father) may sometimes lead to the identification with the wrong parent. This occurred to Frank (9), whose father, a weakling and alcoholic, was mostly absent, and whose mother was energetic and "masculine in appearance and dress" (*cf.* 9). A similar family situation existed in the patient of Deutsch (25): "His mother was a dominant person; his father a lenient type. Mother made all the decisions and liked him best when he was submissive." Thus this man could keep his mother's love only by "feminine qualities", which eventually led to his female identification and transvestism.

THE PSYCHOANALYTICAL THEORY

All psychoanalytically-minded writers base their interpretations of transvestism on the teaching of Freud (*cf.* 36, 37, 38). Their point of view may be summarized thus: transvestism is an attempt to overcome the fear of castration by creating an imaginary phallic woman, and subsequent identification with her. Fenichel, for example, gives this symbolic formula of transvestism: "Phallic women exist; I myself am one" (*cf.* 32). In another paper he elaborates this idea further: "The fantasy of a phallic girl is a substitute for a phallic exhibition which is inhibited by castration anxiety, and is composed of the two kinds of 'castration denial'. I keep my penis by acting as though I were in fact a girl." "Girls are really no different from myself." All this is condensed into the symbolic equation: "I = my whole body = a girl = the little one = the penis." Sadger (89) reconstructs the train of thought of a male transvestite thus: "When I put on my mother's dress I feel as if I were she herself, and so could arouse sexual feelings in my father, and possibly supplant her with him." Böhm (18) uses almost an identical picture: "In the clothes they put on, they represent the mother with the penis."

The inability of a transvestite to renounce the idea of a phallic woman was clearly demonstrated by a classically "transvestite" dream of the 21 year old patient of Peabody (82), "in which he looked under a girl's skirt and saw male genitalia." The same man for a long time "had no clear idea of the difference between male and female anatomy and frequently fantasied going into a ladies' room dressed as a woman in order to see the difference." According to Peabody, "The late date at which he discovered the difference between the sexes, suggest the extreme difficulty he had in consciously accepting the lack of penis in the female."

This fear of castration and its denial through creation of a phallic woman is often precipitated by an exhibitionistic behavior of the important female figure in the transvestite's early childhood, representing most often his mother or his sister (*e.g.* some cases of Bender and Paster, three little patients of Friend; similarly in the patient of Peabody: "Early memories revealed a mixed feeling of revulsion at the accidental" (?) "sight of his mother's genitalia when he was four or five.") Bak (5) also emphasizes the disturbance of the mother-child relationship, resulting in an alternating identification with the phallic and penisless mother, with the corresponding split in the ego. Greenacre too stresses the splitting of the ego: "It is an extraordinarily strong castration problem of the phallic-oedipal period which . . . causes regression and splitting of the ego."

Gillespie (40) assumes "a specific modification of castration anxiety, determined in its form by earlier pregenital, and especially, oral developments." He accepts the splitting of the ego, on lines defined by M. Klein (63), and adds, "What characterizes preversion and makes it different from neurosis or psychosis—is a special technique exploiting the mechanism of splitting of the ego, by which the pervert avoids psychosis, since a part of his ego continues to accept reality and behave fairly normally in the nonsexual sphere."

The picture of a phallic woman, based usually on "a close visual contact" (44) with the mother, or with a sister, may sometimes turn into a *caricature.* Thus the patient of Greenacre, when a small boy, often "dressed in his mother's clothing and paraded in front of a mirror in order to make fun of her." The same ludicrous distortion of a female figure took place in the transvestite play of children studied by Friend, where "there was really seen the conception of caricature of the phallic woman, as well as the underlying denial of sexual difference between patient and mother, sister or girl figure."

In conclusion it may be interesting to recall the drawing of the transvestite patient of Grotjahn, which represents a classical illustration of the psychoanalytical theory of transvestism as a refutation of castration fear. There "the castration is symbolized, but . . . is clearly conceived as a literal castration denoted by the bleeding vagina and testes without a penis. In typical transvestite fashion, the symbolic denial of castration is already implied: the testes may be found behind the curtain (under the clothes). The woman's clothing becomes a substitute for the missing penis" (Grotjahn). Thus "the negation of the woman's castration is magically achieved by the phallic equivalence of her clothes . . . The patient has illustrated the transvestite's dilemma: how to accept the "fact" of castration and deny it too" (Grotjahn).

MULTIFACTORIAL THEORIES

Some writers show a multifactorial eclectic approach to the causation of transvestism. Thus Ellis, "On the psychic side" cautiously accepts the psychoanalytical "identification with woman"; on the "physical side" he blames the "imbalanced endocrine glands", perceiving in transvestism a condition allied to "eunuchoidism". Tennenbaum sees the origin of female transvestism mainly in psychological conditioning, but male transvestism he interprets in almost-analytical terms of "identification with a sweetheart" (?) "or wife" (?). East (28) also uses similar concepts in his etiological explanations. Benjamin believes that "A constitutional predisposition is essential; then comes adverse psychological conditioning followed by the respective syndromes." Petritzer and Foster (83) also show a dualistic approach. "The psychodynamics of . . . transvestism are to be sought . . . in the indoctrination and identification with . . . mother-figure", but "It is reasonable to assume that bio-physiochemical factors based on chromosomal imbalance play a role in creating the transvestite picture. . . ."

SOME NON-SEXUAL EXPLANATIONS

"Janett Thompson" regards transvestism not as a sexual deviation, but as an unusual pattern of behavior, assumed by certain individuals for the sake of some non-sexual motives. "There are endless instances in history of men who have taken to female attire and women to men's clothing—for love, political gain, religious reasons etc., as well as for feelings of inadequacy or dissatisfaction in their own sex role." Such view is not generally acceptable. Although there is no doubt that there are some pseudo-transvestites of this type, it would be a misnomer to apply to them the term "transvestite," by which a strictly defined sexual deviation is known and named.

Probably very rarely an entirely accidental factor can play a precipitating part in the origin of transvestism. Thus Gutheil reports, "One of my female transvestites had an "Ugliness Complex" as a woman. She found that by cross-dressing she was able to appear as a fairly handsome man. This factor was found to play a part in the pathogenesis of her paraphilia."

THE QUESTION OF FEMALE TRANSVESTISM

The above quoted views are largely concerned with male transvestism, while female transvestism is rarely mentioned. This is due to the fact that transvestism is a typically male sexual deviation. If the psychoanalytical view (that transvestism represents a symbolic denial of castration fear through creation of a phallic woman) were accepted, there could be no place for female transvestism, which Fenichel regards as "a displacement of the envy of the penis to an envy of masculine appearance," and which to a transvestite woman "may give the illusion that spectators might believe in the existence of such a penis." Similarly Barahal sees in female transvestism only "a drive for masculinity." Thus "Janett Thompson" remains isolated in his view that "transvestism occurs in both the male and the female," and that "the causes and dynamics and problems faced are very similar."

SYMPTOMATOLOGY

THE CLINICAL PICTURE OF TRANSVESTISM

This may be divided into two types, according to the intensity of symptoms.

The milder form of transvestism is usually confined to mere desire to wear the dress appropriate to the opposite sex, though the "feminine" tendencies of male transvestites may betray themselves by the choice of occupation and hobbies, by feminine mannerisms, and by a general feminine atmosphere in their homes. However, in the great majority of cases the abnormality of the behavior is usually confined to cross-dressing only.

Most transvestites belong to this form of transvestism. They are usually capable of controlling their behavior in such a manner as to avoid criticism and disapproval of their social group. In their daily life they may present no peculiarities in their behavior and dress, and their secret and unusual activities may remain often "unsuspected even by those nearest to them" (Ellis).

This compromise between the urge to cross-dress and the prohibitions imposed by society is achieved in one of two ways: 1, either by permanently wearing one particular part of female garment, most often panties, under their usual male suit (*e.g.* three patients of the present writer; the patient of Wyrsch); or 2, by a complete dressing as a woman, though only for a short period at a time, usually in the evening, in the privacy of their homes (*e.g.* both patients of Bloch, patient "W.W.I." of

Hirschfeld, some patients of Ellis). The latter solution is particularly often sought by narcissistic personalities, manifesting the "Mirror Complex" (Gutheil), *i.e.* the self-admiration in front of a mirror, whilst in a female dress (*e.g.* our patients "A" and "D"; the patient of Grotjahn, some patients of Hirschfeld), often accompanied by auto-erotic practices (*e.g.* our patient "D"; the patient of Shankel and Carr (90); some cases of Hirschfeld; some of Ellis).

Many transvestites, like many homosexuals, often possess a superior intelligence (*e.g.* our patients "A", "C" and "D"; the patient of Delay), and may show some artistic aspirations and abilities (*e.g.* Delay's patient played the piano; the one of Grotjahn's was a painter). These sometimes may provide some of them with a vicarious outlet for their transvestite cravings. (For example, our patient "A" used successfully to play female characters on the stage.)

The more active form of transvestism. Here the longing for cross-dressing may amount to an irresistible urge, resembling an obsessive-compulsive neurosis. Then "It is a drive, or a need, easily as strong, as any organic drive, and must be yielded to, if the victim is to avoid unbearable tension." (Karpman, 59). This drive often shows a fluctuation in its intensity, and in some patients a considerable periodicity (as in our patients "B" and "D"; in case No. 3 of Bürger-Prinz (19, 20).

The transvestites of this type are more active, and often "take up the fight against society" (Hamburger). Yet they would never ask for, nor agree to, the anatomical "change" of their actual sex (*e.g.* our patient "C"). They want to play the part of a member of the opposite sex merely by assuming the dress, the manners and some ways of behavior, which they regard as characteristic of this sex. However, what such an individual creates is not a representation of a woman, but a poor imitation, or even a caricature, of her. To "be a woman" simply means for him "to wear beautiful dresses and high-heeled shoes, to use make-up" and "to attract glances of admiration." Such patient often assumes a female given name (*e.g.* the patient "Ottilie" of Hirschfeld), and wants to be addressed as "she." Some good examples of this Hirschfeldian "Transvestite in Name" may be found in the patients of Guiraud, of Krafft-Ebing, and the virile transvestite girl, case "D" of Bättig.

In some cases the exaggerated desire "to be a woman" may lead to such para-delusional phenomena as the claims of having "menstruation." Some examples: The patient of Krafft-Ebing claimed to have "vicarious menstrual bleeding" from his nose, gums and rectum. One of Hirschfeld's patients wrote in his autobiography, "I am called "Ottilie" (*N.B.* this is a feminine version of the German male name "Otto"). "Every month I have 'my menstruation' . . . intestinal bleeding which lasts up to eight days" (53). Bing's (14) patient claimed to have periodic bleeding from the rectum, Binder's case "D" from the urethra. Thürlimann's (97) Case No. 2 reported "menstrual bleeding" from urethra and anus. Glaus's (41, 42) homosexual transvestite alleged similar "menstrual bleeding." An identical claim of having "menstruation" was raised by the patient "Y" of Bürger-Prinz, and by the patient of Olkon and Sherman (80). The wife of our patient "A" reported "a regular menstrual bleeding from his penis."

Some male transvestites go still further in their female identification, desiring to be able to bear children. As an example, our patient "A" admitted in his autobiography, "When the children were coming, I envied my wife the fact that she could bear them. I would willingly have done so, if possible." Case 8 of Bürger-Prinz wished to be able to bear children. One of Hirschfeld's patients, a 25 year old teacher, entertained fantasies of conception and child-birth in his day dreams. Another of his cases (the confectioner, T.U.) sighs, "I would gladly have borne children, nursed them myself, and brought them up." Hirschfeld (53) met "some transvestites ... who are so dominated by the idea of pregnancy that they dress up to look as if they were pregnant."

Such morbid desires often lead not only to difficulties in social adjustment, but also to feelings of frustration, resentment and unhappiness. A patient of Glaus remarked, "My youth was wasted; shall all my life be a mere Golgotha?" Such complaints convinced Hamburger that a transvestite is subjected to "a continual mental stress that may lead ... to suicidal attempts." However, there is in the whole literature only one case (mentioned by Hirschfeld), where a young man killed himself, dressed in a bridal dress. In two other tragic cases (quoted below), the subjects took their life being afraid of detection, not because they were prevented from playing a transvestite role.

The most frequently cited models of male transvestites are Chevalier d'Eon de Beaumont, and Abbe de Choisy. The celebrated pictures of female transvestites are Lady Hester Stanhope and "James Barry" (*cf.* Ellis). It may be interesting to add one or two less known examples taken from the more recent psychiatric literature.

A male transvestite impersonating a female. A "Mrs. Alice Firth" (Horton and Clarke, 55) was brought up as a girl, pursued only female occupations, and in her twenties married a young man. She was very friendly with the wives of her husband's friends, who never suspected that she might not have been a woman. Five years after her marriage Mrs. F. and her husband were arrested on "liquor charges" (in times of the prohibition in the U. S. A.). In jail there arose some doubts regarding the sex of Mrs. F. Consequently she was medically examined by both authors, who found that she was an anatomically normal male.

Female impersonators of males. 1. A "Captain Tweed" from a transatlantic ship was admitted, acutely ill, into the Sailor's Home on Staten Island in **1904**. During the night he cut his throat. On post-mortem he was found to be a normal female. 2. A head waiter in a principal restaurant in Czernowitz was suddenly taken ill one evening in **1909**. He refused to see a doctor, and in the night poisoned himself. On post-mortem he was found to be female. (Both cases were reported by Hirschfeld.) 3. In the "Daily Express" (21) of October, 1951, there is the "Story of a woman, who" (at the age of 53) "changed her sex and her name, and married a woman." After her death 22 years later the question of her sex was brought to High Court by "his" widow (because of a trust). 4. The "Daily Mail" (22) of March, 1958, reported the story of "Old Chris Williams, the British Legion stalwart, the war-time Home Guardsman, the week-end darts player in the local pub", who for 20 years lived in a small village in Berkshire, where everybody knew him, and who on post-mortem was found to be a woman. "The truth came out after 73-year-old Chris died of a perforated ulcer in the Royal Berkshire Hospital, Reading, last week."

Such cases confirm Hirschfeld's view that "many male and female transvestites ... succeed in maintaining their adopted role unmolested throughout their whole lives," and their "correct sex is only discovered after death, sometimes under very peculiar circumstances." (*N.B.* the real sex of d'Eon was also established only after his death in London, in 1810.)

THE SEXUAL DRIVE IN TRANSVESTISM

Sexual drive is usually weak. According to Gutheil, most transvestites "have little interest in sex. They start late with their sexual contacts and engage in them using paraphilic (transvestite) fantasies." (A good example of this is our patient "C".)

With regard to the *sexual aim*, transvestism may be subdivided into the following types:

The a-sexual type. Here the mere cross-dressing seems to be an end in itself. It completely replaces all other forms of sexual activity and of sexual desires. The dress of the opposite sex provides these individuals with an asexual general feeling of well-being, of relaxation and of satisfaction. Benjamin regards their libido as "cerebral," and says that "it can be aroused and gratified by the fact of "being a woman." Hirschfeld calls them "a-sexual, or sex-less" transvestites.

A classical example of this type of transvestism is the case reported by Flint (quoted after Ellis). This patient, a 21 year old white male, dressed as a maid-servant, was employed by a Boston family. He slept with the servant-girls and attended his mistress in her bath. He never experienced any sexual feelings for either sex, and never had erotic dreams, emissions or erections, although his genital organs were "generously developed."

The auto-mono-sexual type. In this group the cross-dressing, with or without self-admiration in a mirror, seems always to lead to, and to culminate in, masturbation. Occasionally the mere putting on of a female dress brings a pleasurable sensation, often accompanied by erection and ejaculation, without an active masturbation (*e.g.* the patient "U" of Bürger-Prinz). However, most subjects belonging to this group masturbate whilst in a female dress (*e.g.* our case "D"; the patient of Waggoner and Boyd (98), the patient of Deutsch). Wilson's (101, 102) transvestite stated, "I always masturbated in these clothes" (*i.e.* in the clothes of his sister or his mother), and added, "I had associated my desire to be feminine with masturbation." Such statements and observations led Karpman to the conclusion that transvestism was often a form of stimulation in heterosexually inadequate individuals, to obtain a fuller gratification during masturbation. A similar view was expressed by Dukor.

An unusual "splitting" of the male and female components of his psycho-sexual make-up occurred in the patient of Peabody, who used to perform auto-fellation, an "attempt . . . to have intercourse with himself" (82).

The hetero-sexual type. The study of literature leaves the impression that most transvestites in the Western type of culture belong to this group. Benjamin also thinks that, "Transvestites . . . are in the majority heterosexual." Yet Hirschfeld, an authority on this subject, gives the following approximate distribution of the sexual expression among male transvestites: "about 35 per cent . . . are heterosexual, and an equal percentage homosexual, while about 15 per cent are bisexual. The remaining 15 per cent are mostly automonosexual, but also include a small proportion of asexuals." Hamburger's figures are very similar (*i.e.* 39 per cent heterosexual, 40 per cent homosexual (19 per cent latent, and 21 per cent overt), and 21 per cent bi-, auto-, and a-sexual).

It is interesting to add that a heterosexual act in many of these individuals is sometimes conditioned by their being in a succumbus position during coitus (*e.g.* Bürger-Prinz's Cases 4 and 7) and is often accompanied by passive and inverted copulatory fantasies, suggestive of latent homosexual trends. A good example of this is our patient "B".

The homo-sexual group. Hirschfeld and Hamburger alike found that the homosexuals among male transvestites constitute about one-third of all cases. Although this figure seems to be high, it is extremely low in comparison with the percentage of homosexuality in female transvestites, who (according to Hamburger) are literally all homosexual (18 per cent with latent, 82 per cent with overt homosexual behavior). The figure arrived at from the study of reported cases is identical.

There might be several explanations of this fact. 1. Perhaps female transvestites, being temperamentally less stable, are less inhibited in a free expression of their homosexual tendencies. 2. Perhaps female transvestism is in fact only "a screen for homosexuality" (Stekel's phrase), or, 3, possibly the fact that in most Western countries male homosexuality is punishable by law, whilst the female is not, acts as a deterrent for an overt expression of homosexual trends in many a male transvestite.

The bi-sexual type. According to Hirschfeld, this type contains about 15 per cent of all transvestites, though the study of literature may suggest that their number is much smaller than that. A typical example of a bisexual transvestite was the patient of G. Thompson, who at the age of seven engaged in homosexual activities with his elder brother; between 12 and 14 he cross-dressed and played with dolls; he married at 31, had two children, and continued with his homo- and hetero-sexual activities.

TRANSVESTISM AND OTHER SEXUAL DEVIATIONS

All sexual aberrations "usually overlap and merge to a more or less extent" (**80**). Hence almost in every case of transvestism there are certain features characteristic of some other sexual anomaly. This fact indicates a close relationship among various sexual deviations, but by no means disproves their independent character.

It was Hirschfeld who first advocated, in 1910, a complete separation of transvestism from all other sexual aberrations, and sharply criticized all those authors who failed to see in it an independent sexual anomaly. He did not spare even the most prominent of them: "Krafft-Ebing himself... remained ignorant of the true nature of the phenomenon" (*i.e.* of transvestism). "He saw in it, like most authors before him and after him, nothing but a variant of homosexuality, whereas today we are in a position to say... that... transvestism... is a condition that occurs independently and must be considered separately from any other sexual anomaly."

Hirschfeld's view has been generally accepted, and nowadays almost all writers regard transvestism as an independent sexual deviation. However the complexity of the clinical picture in transvestism immensely complicates its delimitation. For example, one of Hirschfeld's transvestites developed the following very elaborate, verbalized homosexual and sado-masochistic masturbatory fantasies: "You, sweet man, I want to belong to you. Kill me, but let me be your wife." Such cases as this forced some authors to see in transvestism a conglomeration of various sexual deviations. For instance, Gutheil regards it as a "result of six psychopathological factors. They are: 1, latent (or manifested) homosexuality with an unresolved castration complex; 2, the sado-masochistic component; 3, the narcissistic component; 4, the scoptophilic; 5, the exhibitionistic; and 6, the fetishistic component. In every case all tributaries are represented in varying degrees." Wiedeman (**100**) points out the presence of the "features of fetishism (overvaluation of female clothing), homosexuality (desire to be a woman), and masochism." Fenichel stresses 1, the masochistic, 2, the exhibitionistic, and 3, the fetishistic tendencies in transvestism.

Some traits of other sexual deviations, met frequently in transvestism, will be now reviewed and illustrated by brief examples from actual cases, taken from the recent literature.

The homosexual traits in transvestism. They may be manifested either as latent homosexual tendencies, or as an overt homosexual behavior. The latter is usually met in transsexualists and in female transvestites; male transvestites rather show latent homosexual tendencies, often only in their dreams and day-dreams (*cf.* 69), or their coital or masturbatory fantasies (as our patient "C"). For instance, Karpman's patient had such homosexual (though inverted) dreams. "I have dreamed of women, who played the role of a man in relation to me as a woman... have dreamed of them having a penis and me having a vagina and able to receive the penis... The masculinized women in my dreams are women transvestites, never a full woman, never a full man." Our patient "B" had similar inverted homosexual fantasies during coitus with his wife.

However, *overt* homosexual trends were also described in male transvestites; for example, in cases No. 2 and 4 of Thürlimann; cases A, C and F of Bättig; case 9 of Bürger-Prinz; the patient of Delay, and some other cases.

A few words should be devoted to the problem of transvestite children with overt homosexual behavior. Of 23 homosexual children, studied by Bender and Paster, 12 were transvestite. They exhibited their transvestite tendencies not only by cross-dressing, but also in their manners and general behavior, as well as in their explicit verbalizations, expressing their desire "to be a girl," respectively "to be a boy." They all came from broken homes, and their age varied from 5 to 13, the mean being 8 years and 3 months. The rarity of this phenomenon may justify a brief summary of one case. John, aged 8, played only with dolls, loved the company of girls, and considered them superior. He wanted himself to be a girl, wore shirt over his pants and proposed marriage to men. He fantasied, "My mother was a little man, when she was young, but she was nice, and God changed her into a woman. I dreamed that God was going to change me into a little girl." He fantasied also that he had children, and would say, "I want to go home to my husband." Phenomenologically he resembles the psychotic patient of Liebman (66), and there is also some similarity of the psychodynamics involved.

The fetishistic traits in transvestism. Abraham's (2) patient, a 22 year old college student, well illustrates how closely interwoven are often various sexual perversions, and how one single case may present a variety of different components. He was a fetishist and a transvestite, with masochistic and sadistic features, and with scoptophilia. He wrote in his autobiography, "I have often wished I was a woman... I often long to wear women's clothes, stays and shoes." In fact, he not only longed, but often put on his mother's stays and laced himself tightly, deriving a sexual pleasure from this complex, transvestite, fetishistic and masochistic behavior. He also showed some sadistic trends. "If I see women and girls tightly laced and picture to myself the pressure of their corsets on their breasts and body, I can get an erection;" and scoptophilia (which probably replaced his repressed osphresiolagnia; *cf.* Wilson's papers): "Looking out for elegant shoes or tightly laced waists became his most important sexual activity. This interest occupied the chief place in his vivid day-dreams. At night he had frequent erotic dreams about stays, tight-lacing, etc". (2).

The narcissistic and exhibitionistic traits in transvestism. Narcissism is best exemplified in the phenomenon of "The Mirror Complex". There follow some examples of this. "In this attire" (*i.e.* in female clothes) "I felt divinely. I then stepped in front of the mirror... my face blushed, shivers of joy rushed down my spine" (Case 7 of Hirschfeld). "I spent my nights sitting in front of a mirror and put on my mother's dresses... " (Hirschfeld's Case 12). Our patient "D" may sit for hours in front of a mirror, dressed and made up as a woman. A most intense narcissism was demonstrated by the patient of Grotjahn, who for hours would admire himself, dressed as a woman, in a mirror, or watch a film he took of himself when dressed and made up as a woman. "He then loved himself with such narcissistic intensity, that he felt almost satisfied" (45).

The exhibitionistic tendencies in transvestites are manifested in their desire to appear in public places in female clothes (*cf.* our patient "D"), and in their craving to be photographed in female garments, particularly when they are doing embroidery, or sewing, or performing some other "typically feminine" type of work. They like also to have their pictures taken in a female evening dress, or wedding dress, or when they are dressed as servant girls (*cf.* photos in the paper of Wilson, and in the book of Cauldwell).

The masochistic traits in transvestism. They were clearly exhibited in Alexander's (3) patient. "A transvestite of 40 finds sexual satisfaction when, masquerading as a servant girl, he is able to serve a severe mistress. His mistress must forbid him to wash himself, until his hands become black with grime. The idea of grimy hands is of itself pleasurable to him and forms the kernel of his masochistic fantasies." He also manifested an unusual coprophilic interest. "At seven or eight years of age he frequently smeared himself with feces and while in this state masturbated." (One can see here also some osphresiolagnic tendencies, exemplifying Wilson's "unsuccessful olfactory repression," which he regards as a "feature common to fetishism and transvestism"). His masturbatory fantasies were definitely masochistic. He fantasied he was being buried by his mother "for punishment" in a dung heap. (According to Alexander, "It is an intra-uterine phantasy, which nevertheless means incestuous coitus. This coitus fantasy has a clear masochistic coloring (punishment) and at the same time an anal-erotic one (dung-heap)." The patient showed also some homosexual features: "... in his masochistic relation to his mother are included the passive homosexual wishes for his father... A series of dreams showed us... that his passive homosexual wishes, which had a strong masochistic coloring, were transferred from man to woman, to his stern mistress—who is nothing more than a man in a disguised form" (3).

Another example of masochistic tendencies in transvestism, illustrating Gutheil's "Martyr Complex", is Hirschfeld's Case 12, who reported, "I imagine that I am forced by a woman to dress as a woman and expose myself to the teasing and derision by those who are watching me." Gutheil believes that an operation so intensely desired (castration) "also fulfills in part the tendency toward expiation of the sexual guilt." The ward expiation of the sexual guilt." The same meaning is applied to tatooing, which "... represents an attenuated form of the desired or performed castration, an act of aggression against the patient's own body...".

A rare type of masochistic auto-erotic transvestism is the *Strangulation Masochism.* "The masturbatory practices of such individuals consist of dressing and making up as woman and then performing an act of self-strangulation leading the patient to the threshold of complete asphyxia. In many of these cases the act of hanging is imitated, the constriction of the neck being used as a sexual stimulant. Once orgasm is obtained, the patient relaxes the constriction. Unfortunate accidents often occur..." (48). A typical example of this phenomenon is the 17 year old boy reported by Shankel and Carr. He alleged, "It always gave me a nice erection, and than I would masturbate." He started his self-hanging "at the age of 10 or 11", and did it "pretty often over the years."

TRANSSEXUALISM

Everything said about transvestism is applicable to transsexualism, with the following reservations. 1. It is likely that all transsexualists are homosexual. 2. Transsexualism is a typically male sexual deviation, and there is only one case which might be regarded as female transsexualism (the homosexual "transvestite" of Dukor). 3. The morbid desire "to be a woman" is much deeper in transsexualism, and turns soon into an obsessional urge for a conversion-operation.

Clinically transsexualism may be divided into mild, severe and intermediate types.

In the mild or benign form the patient may be satisfied with a partial "operation" (or self-mutilation), without demanding an official change in his civil status (*e.g.* the soldier described by Wyrsch; the patient of Grotjahn); or a partial "operation" (or self-mutilation), and an obsessive demand for an official recognition as a member of the opposite sex (*e.g.* the patient of Glaus, the patient of Dukor), but without the desire for any further surgical intervention. There seem to be two possible explanations of the mechanism involved. Either the "operation" may represent an emotional shock, bringing the patient back to reality (similar to ECT); or it may act as a strong emotional cathexis, a symbolic expiation of guilt (like an attempted suicide), thus relieving the aggressive and hostile feelings towards his own genitalia. The rationale in the second group may be the gaining of a stronger "bargaining position" with authorities, presumably with the following line of reasoning. "Look here, I am physically no more a male; now it is easier for you to recognize and to sanction it."

In the severe or malignant form the individual would be satisfied with nothing else but a complete conversion-operation and an official recognition of his "new" civil status (*e.g.* patient "B" of Bättig; patient of Glaus). When frustrated in his obsessive demand for the surgical "correction" of his "wrong" sexual organs, a transsexualist may attempt to mutilate himself (*e.g.* the patients of Glaus, of Grotjahn, patient "A" of Bättig, etc).

The intermediate type of transsexualism probably consists of cases of very slowly developing transsexualism, presenting for years a typical picture of a "mere" transvestism, and turning eventually into transsexualism, often under the impact of publicity on cases of the "change of the sex" (*cf.* some patients of Bättig). The other possibility is that these patients are cases of true transsexualism, which became "arrested" under the impact of a partial conversion-operation, or a partial self-mutilation (*e.g.* the patient of Wyrsch).

The extreme rarity of such cases justifies a brief survey. Only three such cases are known (though they were not recognized as such by authors who reported them). Dukor described a "virile homosexual transvestite," who lived with another female and played the part of the "male" in their homosexual relationship. She had her breasts amputated by a surgeon, as a preliminary step to a planned complete conversion-operation, but changed her mind and started a legal compaign, trying to achieve the change of her given name and her civil status without further surgical interference. A similar "arrest", or spontaneous self-limitation of the morbid process, occurred in a male "transvestite" reported by Wyrsch. This 49 year old professional soldier, married, with children, himself amputated his penis. After his self-performed peotomy he had no more desire for further "femininizing" operations. He was a permanent partial "transvestite", continually wearing female pants inside his military uniform. The patient of Grotjahn amputated one of his testicles, and, putting on the bleeding wound a menstrual pad, found, disguised as a woman, his way into a female ward. Yet he contented himself with this partial self-castration, and occasional complete cross-dressing, culminating in long solitary walks in female dress. He was not homosexual, and did not wish any further "operations." It seems that his auto-castration (as in Wyrsch's patient) brought his transsexual process to a stop.

MISCELLANEOUS

AGE OF ONSET OF TRANSVESTISM

Although the patient is seen by a doctor for the first time usually in his late adolescence or early adulthood, the beginning of his transvestite tendencies is to be sought in his early childhood. There is a general agreement on this point. Anthropologists (*e.g.* M. Mead; Ford and Beach, 34), medical men and psychoanalysts alike, date the origin of transvestite trends from early childhood.

Friend was the only writer to witness the development of a case of transvestism, so to say *in statu nascendi*, occurring before the phallic period. Bender and Paster had the opportunity of studying a group of homosexual children, whose average age was just over eight years. In many of them transvestite trends were already well established, which suggested that they began at a much earlier age.

It may be useful to divide transvestite phenomena into the *subjective* claims of patients that they "always wanted to be a woman," and the *objective* manifestations of transvestite behavior, such as cross-dressing, avoiding the plays and games appropriate to one's own sex, and adoption of mannerisms and behavior characteristic of the opposite sex.

Such subjective claims as "I always dearly wished to be a girl" are referred by patients to a very early but unspecified age, usually "ever since I can remember." However, on closer scrutiny it may be established that very many transvestites discover in themselves this desire "to be a woman" only after reading of cases of "change of sex" in newspapers and popular magazines (*e.g.* our patient "C"; almost all patients of Bättig). Then their fantasy begins to play with imagining themselves in "beautiful female dress, high-heeled shoes", etc. A wishful falsification of memory takes place, the patients begin to recall and to misinterpret various insignificant incidents in their childhood, till they finally firmly believe that "ever since I can remember, I always wanted to be a woman." (The incessant progress of these emotionally overvalued ideas resembles the relentless development of delusions in paranoia.)

It should be easier to verify the first objective manifestations of transvestite behavior, and in particular the first attempts at cross-dressing. Yet even here one has almost entirely to rely on the information supplied by the patient himself. Thus also this part of the history is neither very reliable nor objective.

Here are brief reviews of a few cases, in which the transvestite activities began at a more or less well defined age. Our patient "A" alleged that at the age of three he vigorously resented when his girl's skirts were taken away, and he had to wear trousers. Thus his transvestite desires seem to have been established even at this very early age. The patient of Petritzer and Foster began to cross-dress at five; the one of London at the age of six. Our patient "B" dates his first conscious desire to be a girl and to dress like a girl at the age of seven, our patient "D" at 12. In one of Bürger-Prinz's cases the transvestite tendencies became manifested at three, in two other cases at ten, in remaining cases "in early childhood." All patients of Warden and Marsh "began at an early age fantasying that they were girls." In Karpman's patient the first manifestations of transvestite behavior occurred at the age of ten. Berman's case "placed the onset of his interest in transvestism at the age of 11." Of Ellis's own cases three began to dress in female garments in their early teens. Bättig's Case "A" began to cross-dress when he was 11; his other cases "at a very early," but unspecified age.

There are only a few reported cases, where the transvestite tendencies manifested themselves for the first time in the relatively late teens (for instance in Wilson's, and of Ellis's patients, at 16.) Cases where transvestite tendencies become manifested for the first time in adulthood, in connection with a psychiatric illness, (*e.g.* our patient "C"; the patient of Guiraud and many others), are dealt with in a different paper (*cf.* 69).

PROGRESS AND OUTCOME OF TRANSVESTISM

Only a few writers commit themselves on the question of the progress and outcome of transvestism. Thus there are few views on this subject to be reviewed.

As far as it is permissible to imply from Krafft-Ebing's term "metamorphosis sexualis paranoica," he probably regarded transvestism as showing a continuous and relentless course (similar to the course of paranoia), without a spontaneous termination. In Bürger-Prinz's view, although the identification of a transvestite with a woman undergoes a continuous development, modifications, and refinement, it never comes to an end.

The present writer suggests the following hypotheses.

1. *Transvestism* is self-limiting in its course and its aim. Although showing considerable differences in its intensity from case to case, and some temporary fluctuation in each individual case, it usually confines itself to a mere cross-dressing, without any desire for the surgical "change of sex." The therapeutic implication of this fact is clear: a case of simple transvestism can be helped by psychotherapy, combined with an understanding and permissive attitude on the part of his environment (*e.g.* Bürger-Prinz's Cases 2, 3, and 7; many cases of Hirschfeld; our Cases "A" and "D").

2. *Transsexualism,* on the other hand, shows a progressive, malignant course, and any permissive gesture or concession always brings some new, more far-reaching, and more morbid demands (*e.g.* Bürger-Prinz's Case 9; the case of Glaus).

3. The few *intermediate cases* seem to belong to two clinically different subgroups. In the first group a hitherto typical case of transvestism becomes acutely disturbed (usually under the impact of some "sensational" literature), turns, as it were, malignant, and degenerates into a full-blown picture of transsexualism with its gloomy prognosis (*e.g.* some cases of Bättig). To the second group belong the cases of transsexualism, turning "benign" after an initial mutilation (*cf.* the patients of Dukor, of Grotjahn and of Wyrsch), and showing a spontaneous arrest, just on the threshold of a full conversion-operation.

4. Time, as such, does not appear to have any therapeutic, or limiting, effect either on transvestism or on transsexualism. The same applies to the factor of age. "The problem is alive in men almost aged 60" (Hamburger), and our patient "A" was 54 when he wrote in his autobiography, "All through the years the feeling of really being a woman has persisted..."

FREQUENCY OF OCCURRENCE OF TRANSVESTISM

There are no statistical data on the incidence of transvestism. Hence the views on the occurrence of this phenomenon are not only diverse but highly speculative. Thus one group of authors believes that the number of transvestites is negligible. "Genuine transvestism is certainly an exceedingly rare affliction... We are in personal contact with five Danish transvestites in a population of nearly four million inhabitants" (Hamburger). Similarly Benjamin thinks that transvestites and transsexualists in the U.S.A. "... are hardly enough to constitute a problem ... even if restrictive laws were relaxed with the help of medical certificates." Another group of writers is of the opinion that transvestism is not at all as rare as it may appear. For instance, Wilson says that "transvestism is quite a common perversion," and Ellis states that in his experience transvestism comes next in frequency to homosexuality. Within the last three years we have personally treated four male transvestites in Bristol, a city with half million population. Thus our figure is over six times higher than Hamburger's. Yet both figures have no meaning and no value. Nobody knows, how many transvestites, apart from our four patients, live in Bristol alone. The same lack of information applies everywhere.

The paucity of the literature on transvestism may be easily explained. The great majority of transvestites usually succeed in achieving a fair social adjustment, and are never discovered, or even suspected. A further reason for the scarcity of reports on transvestism is "the extreme reluctance of transvestites . . . to undergo any treatment that . . . would remove the perversion and deprive them of their perverse gratifications" **(100)**.

With transsexualists the situation is somewhat different. They are much more dynamic and restless, and do not try to conceal their desires. On the contrary, they molest their doctors (often using threats of committing suicide), demanding to have a conversion-operation. At a later stage they are usually in conflict with society (represented by legal authorities) and demand an official recognition of their "human right to be a woman." Thus they easily achieve publicity, and soon find their way into the lay press and the psychiatric literature. Yet even here an attempt to procure approximate figures on the number of transsexualists is doomed to a failure.

SEX DISTRIBUTION IN TRANSVESTISM

It is equally impossible to collect any reliable data on the incidence of transvestism in the two sexes. The study of literature shows that there is a definite preponderance on the part of the male sex among the transvestites, and the majority of writers express the same view. Benjamin believes that "Transsexualism as well as transvestism are decidedly more frequent among men than women," and Kinsey states that in the U.S.A. ". . . there may be a hundred anatomic males who wish to be identified as females for every two or three . . . anatomic females who wish to be identified as males." (N. B. Kinsey does not support this statement with data.)

Yet, as has already been pointed out, the discriminative social attitude towards each sex, in our culture, is greatly responsible for the obvious preponderance of male transvestites in the psychiatric literature. For example, in most Western countries the male transvestite may be arrested and charged for wearing female dress in public, whilst female transvestism is tolerated by custom and by law. Therefore the male transvestite, feeling the hostility and the contempt of society (and often also of his wife, as in our patients "A", "B" and "D"), is virtually compelled to seek medical advice, with the risk of being reported as "another case of male transvestism." The female transvestite, even if she were a notorious homosexual (*e.g.* the patient of Dukor) will quite certainly remain unmolested for her transvestism and her lesbianism, unless she commits an offense of some other punishable nature, as was the case with the patient of Redmount **(87)**.

Moreover, thousands of transvestite females may choose an occupation (*e.g.* of bus conductresses, gasoline station attendants, station-women, farm or stable workers, etc.), which would provide them with an excuse for customarily wearing a male "uniform," without exposing them to any suspicions.

Men, on the other hand, do not have similar, socially approved channels of outlet for their transvestite tendencies. Though Wilson alleges that "there is a considerable freedom of opportunity to act out certain degree of transvestism, particularly in theatrical and entertainment world," only a negligible number of male transvestites may occasionally satisfy their transvestite cravings by impersonating female characters on the stage. Wilson himself quotes only two such instances. One could add, perhaps, one or two more examples, but that is all. Here again, female transvestites have more possibilities of acting out their desire for cross-dressing by impersonating young male subjects on the stage. For instance, all roles of pages are, as a rule, performed by women. Some parts of this kind are designed to be filled by women (*cf.* the role of Siebel in Gounod's "Faust", written for soprano). Thus even on the stage, male transvestites are at a disadvantage. Finally it should be remembered (*cf.* the section on symptomatology) that transvestism is a typically male sexual deviation and that the female cross-dressing is etiologically something entirely different.

TRANSVESTISM AND PSYCHONEUROSIS

There are several kinds of relationship between transvestism and psychoneurosis. When a transvestite is prevented from satisfying his urge, he may react with a severe psychoneurosis, or even a psychosis. Here are examples of both types of reaction. Each time our patient "A" tried to desist from his transvestism, he developed an acute neurotic depression. A patient of Peabody, a 39 year old married man with three children, "was precipitated into an acute psychosis, with temporary regression to an infantile level" under similar strain.

A psychosis in a "latent" transvestite may break down his inhibitions, and thus precipitate an overt transvestite behavior. The patient of Liebman developed transvestite and overt homosexual behavior during his schizo-affective episode.

There may be an accidental combination of transvestism and psychoneurosis (or psychosis), as, obviously, a transvestite subject is not more immune from a mental illness than any other individual (*cf.* our patient "C", 69).

The same holds true for the infrequent combination of transvestism and psychopathy. This clearly is not of the order of cause and effect. The only possible etiological connection is one of inefficient moral and social inhibitions in a psychopathic personality, causing any kind of undesirable behavior and antisocial trends to emerge freely. It may be said that what happens in psychosis (the breaking down of inhibitions) only temporarily, is exhibited in psychopathy permanently (as there is a permanent lack of inhibitions). There are very few cases of transvestism among psychopaths. (Among others: our Case "B"; Aubert's (4) Case "A"; the patient of Delay, and perhaps one or two more.)

TRANSVESTISM AND HEREDITY

All writers agree that transvestism is not hereditary, and there are no cases on record reporting definite overt transvestite behavior among the ancestors of transvestites. There are, however, two cases, requiring some elucidation. Ellis suspected that the father of Case T. S. "had a latent impulse of this kind and near the end of his life, when in a delirious condition, he endeavored to put on his wife's clothing." Yet this instance of an old man, "near the end of his life," trying to put on his wife's clothing, "when in a delirious condition," can hardly be regarded as a proof of a latent transvestism. The mother of a six year old transvestite and homosexual boy, described by Bender and Paster, probably had some transvestite tendencies. She was said to be "masculine in appearance and dress." However, there is no other information about her available, and no proof, that she really was a transvestite.

In the literature, the writer has found only two cases of transvestite tendencies among the siblings of transvestites. The sister of the patient of Berman cross-dressed in her childhood. Yet there is only the testimony of the patient himself to this effect. Furthermore, this individual never showed any transvestite trends after her childhood. The sister of Karpman's patient cross-dressed together with her brother in their early teens. Here again we have only the patient's account. The only possible conclusion is that there is insufficient evidence of hereditary transmission or familial occurrence, of transvestism.

THE ROLE OF CULTURAL FACTORS IN TRANSVESTISM

The important part which cultural factors play in the etiology of transvestism may be illustrated by two quotations. "Whether transvestism will occur seems to be a question of cultural recognition of this possibility" (Mead). "Any aspect of sexual behavior ... may be simultaneously determined biochemically, anatomically ... and by ... cultural factors ... like economics, religion, and social organization" (Honigman, 54).

The social attitude toward transvestism varies a great deal among different cultures. Some social groups do not recognize transvestim at all (*e.g.* the Samoans), others cultivate it (*e.g.* Mohave Indians). Certain societies not only openly approve of transvestism but keep the transvestites in a high social esteem (*i.e.* shamans in Siberia). Some cultural groups forcibly procure artificial, quasi-eunuchoid transvestites, used later for homosexual ritual practices (*e.g.* Pueblo Indians).

These are a number of examples of such "compulsory transvestism", usually enforced by tribal tradition. Plains Indians "expected so much of their youth that many were driven to accept woman's dress and a woman's role" ("Janett Thompson"). "Sarimbavy," found among the Movas (the natives) of Madagascar, are usually brought up as girls, because their parents desired to have a girl (Ellis). Ehlers described the "Pu-Mea" (men-women) in Indochina, brought up as women, and living among them. Sometimes they "marry" other men (most often the impotent opium smokers), such "marriages" being socially accepted. Frazer (35) reported that among the Sea-Dyaks of Borneo some men live as women, after a dream regarded "as a call to do it." "Batjih" is an "artificial" homosexual transvestite in neighboring Bali.

In Western culture there is only one example of socially enforced, temporary, transvestism, resembling the Madagascarian "Sarimbavy". In the Island of Maarken in Holland there is an unusual custom to dress boys as girls till they are seven. Jelgersma (57) regards this as a "symbolic castration", imposed by men "to guard against the incestuous tendencies of children among a seafaring population" (Ellis).

On the whole, the attitude towards transvestism is more permissive in the non-Western cultures. For example, Ford and Beach found, among 49 out of the 76 societies under survey, that some form of transvestism is there "considered normal and socially acceptable for certain members of the community".

In the Western culture social opinion on transvestism is different for each sex. There is a considerable lenience for a mild "fashionable" type of female transvestism, in form of slacks, tailored clothes, short hair, etc. On the other hand, there is a marked intolerance towards any manifestation of male transvestism, which is being regarded, too lightheartedly, as an indication of homosexuality. There are, however, some regional and traditional exceptions to this Western concept of "maleness" with respect to the "femaleness" of certain types of garments. Scotch Highlanders wear kilts, for example, without being suspect.

Transvestism is not related to any social class, though it seems to occur more frequently in children from a broken home, as the survey of the social and family background of Bender and Paster's group of sexually perverted children has shown.

THE ROLE OF TRANSVESTISM IN MARRIAGE

It appears from the literature that more than half the heterosexual male transvestites are married and have children. In "Janett Thompson's" experience "by far the larger number ... are married and with children." Moreover, there are not a few among them, who have been married even more than once (*e.g.* Cases 3, 4, 5, 6 and 7 of Bürger-Prinz the patient of London, and some other cases).

Transvestites of both sexes usually marry with an optimistic, though fallacious, hope, that the marriage will "cure" their desires for cross-dressing, and will bring an end to their doubts and uncertainty as to their "proper and true" sex. Not all of these opportunist marriages are lasting. Yet the male transvestite more often makes a fairly satisfactory adjustment, and his marriage has every chance to survive, if his wife shows some understanding and a certain amount of permissiveness for his transvestite cravings (*e.g.* our patients "A" and "D").

It is significant that if divorce occurs in a male transvestite's marriage, it is usually brought about by his wife. The most frequent reason is that she "got tired with his masquerading and all that sort of thing." Yet if a female ("virile") transvestite is divorced, it is again she, the wife, who seeks the divorce, the reason being that her husband's heterosexual approaches are "disgusting," "nauseating" and "unbearable" to her. It appears that the attitude of the wife is always the decisive one in a transvestite marriage. If the male transvestite himself initiates a divorce action, his usual reason is a hostile attitude and a complete lack of understanding on the part of his wife (*cf.* our patient "B"). It is characteristic that all reported divorced male transvestites make a success of their second marriage, finding more sympathy and more permissiveness from the second wife. Our statements find confirmation in Hamburger's figures: he gives the percentage of divorced male transvestites as 23, that of females as 86, *i.e.* a ratio of one to four.

The transsexualists, being virtually all homosexual, are rarely tempted to enter into marriage. There are two cases which seem to contradict this statement (*i.e.* the patient of Wyrsch; Case "Y" of Bürger-Prinz), but they apparently belong to the small "intermediate" group, and not to transsexualism.

TRANSVESTISM AND THE LAW

Transvestism. The official interpretation of the law on this subject in Great Britain was given in an article on "Masquerading" in "The Justice of the Peace" in 1938 (94). Unfortunately the law itself, and its interpretation, is rather complicated. For example, although "the mere wearing of clothes appropriate to the opposite sex ... cannot ... be made the ground of any criminal charge," yet "cross-dressing may, *prima facie*, be evidence of an intention to commit some crime," and "a man, masquerading as a woman in a public place," may "incur suspicion under Section 1 of the Vagrancy Act of 1898," as happened in Dorking, in 1933, when "a man found in woman's clothes was convicted of being a suspected person loitering with the intention of committing a felony." Thus every transvestite, who appears "in a public place" in female garments may be charged and convicted under the Vagrancy Act of 1898.

A similar attitude prevails in other countries, where the legislature is based on English legal concepts. For instance, in the U. S. A., "There is no law which expressly forbids a man from wearing the clothes of a female ... But this fact does not mean that the transvestite will not be arrested for merely wearing the clothes of the opposite sex" under "a wide and broad Disorderly Conduct Statute" (Shervin, 91). Yet, as Benjamin writes, "Only the male transvestite ... is in legal difficulties, for customs and more flexible fashions allow women to dress like men."

In some European countries (*e.g.* in Germany, and in Scandinavian states) there predominates a more permissive attitude towards transvestites of both sexes. The authorities there may allow a male transvestite to wear female clothes in public on a medical recommendation, that this is indispensable for the mental health of the patient. In this way many of Hirchfeld's and also some of Bürger-Prinz's patients were allowed to wear female dress openly (in Germany). In some cases similar permissions were granted on medical certificates in Denmark and in Sweden (Hamburger).

The law and the public opinion on transvestism in societies outside our own cultural pattern varies considerably, according to different customs and different traditions (*e.g.* wearing of trousers by women in the Near and the Far East). On the whole, the attitude towards this problem is there more liberal.

The conversion-operation. The question of the legality of a conversion-operation (involving castration, peotomy, and a plastic operation of perineum with creation of an artificial "vagina"), is in practice reduced to the mere question of castration, for, as Hamburger says, "Surgicoplastic measures in respect of the genitals of castrated persons... have not been forseen by the Law".

In most Western countries castration requires a judical authorization, but the Law is not concerned with cases where some pathological process in the sexual organs (*e.g.* a neoplasm) justifies the necessity of their surgical removal. The view of the Roman Catholic Church on "sterilization" is identical, and was recently restated by the Pope (23). In some Protestant countries (*e.g.* in Denmark, Norway, Sweden) a voluntary castration is legally permissible, "when the patient's sexuality makes him prone to commit crimes,... or when it involves mental disturbances to a considerable degree, or social deterioration" (50). Under certain circumstances castration may also be sanctioned in Switzerland and in Holland. From the fact that such an operation was performed in recent years in this country (Robert/Roberta Cowell in 1953), it may be concluded that in Great Britain a conversion-operation may be authorized in some exceptional cases. In the U.S.A. "there is no law which specifically prohibits a doctor from performing this operation with the consent of the patient. Nevertheless, there is hardly a district attorney in the country, who would not inform a doctor that it would be illegal... to perform such an operation" under the "Mayhew Statute" (Shervin).

We have no information on the legal aspects of this question in social groups outside the Western culture.

The change of civil status. For practical reasons this problem may be divided into two questions; the change of name and of civil status in *transvestites,* who had no surgical interference, and the change of civil status in *transsexualists,* who already had a conversion-operation, or a part of it (usually in form of castration).

The first question concerns a particular type of transvestites, called by Hirschfeld "name-transvestites," whose number is negligible, and no law dealing specifically with this problem is known to the writer.

The second issue has been recently illustrated by one case in Great Britain, where a change of name and of civil status was authorized after a conversion-operation (the already mentioned case of Robert/Roberta Cowell, *cf.* 85). In Switzerland, however, there have been, in the last 30 years, at least five cases where a change of civil status and of name was granted by legal authorities on medical recommendation. (These cases were described by Aubert, Binder, Dreyfus (26), and Glaus.)

It may be of interest to review some verdicts. In the case reported by Glaus, the County Court ("Bezirksgericht") in Kanton "x" decreed, "According to the medical opinion S.J. presents a case of transvestism, where the individual appears to be male in his external structure, but in his psychic disposition is predominantly female. The patient suffers from this deviation; thus the use of a female name is being granted to him." Similar reasons were quoted in two cases described by Dreyfus. A 29 year old male, an ex-serviceman, with normal male genitalia, felt himself to be "a woman", and because of a "potential homosexuality" (?) had a conversion-operation, on psychiatric recommendation. The judical authorities of Kanton "y" granted him a change of civil status, and the permission to assume a female name, basing their decision on two medical certificates that stated that the patient was a case of an *anima muliebris in corpore virile.* After castration, a certain man called Nikolas was given permission by the "Regierungsrat" of Kanton "z" to name himself "Margareth". It was stated in the decree that it had been unbearable to him to wear male attire, and that he would commit suicide if he would not be granted the recognition of the "change of sex" (?), "respectively the permission to wear female clothing" (26). It is rather difficult to feel convinced by such an argument, and one is tempted to paraphrase Gutheil by asking, "If a patient demands that his tongue be removed to prevent him from cunnilingus, should a doctor and a court comply with his request and sanction such mutilation?."

The legality of a homosexual "marriage". It is tempting to mention here the puzzling question of "legally" concluded "marriages" between two homosexual individuals, one of whom is a transvestite. Such "marriages" are recognized in some non-Western societies (*cf.* "Pu-Mea" in Indochina), but they occur also, very rarely, in our culture. As examples: a "marriage" between two (probably homosexual) men, Horton and Clarke's "Mrs. Alice Firth"; "marriage" between two homosexual females, one of whom was a "virile" transvestite, the case described by Redmount; the Case of Wynifred Mary Swan, who at the age of 53 "changed her sex" (?) "and her name" to Wynsley Michael Swan, and "married" another woman, with whom she lived for 22 years until her death (*cf.* 21).

THE TREATMENT OF TRANSVESTISM

Our therapeutic possibilities are still extremely limited. Similar opinion has been expressed by many writers. For example, Glaus says that, "The tragedy in the life of these human beings is innate and inborn and can't be completely remedied in any way, neither by medical nor administrative means." Benjamin admits that, "Medical science is rather helpless in the more pronounced instances of deviated behavior ... This applies to transsexualism, transvestism."

Various kinds of treatment have been attempted in cases of transvestism, though none of them can claim to be really successful.

Psychotherapy. In sexual deviations, as in other fields of medicine, there is considerable controversy as to the usefulness of psychotherapy. Some authors criticize it, while others enthusiastically advocate this approach. For instance, Ostow (81) advises an "intensive, prolonged, classic psychoanalysis" in the treatment of transvestism. Peabody believes that, "Up to the present time, the analytical approach or psychotherapy with emphasis on dynamic understanding and guidance offer the best results."

There hardly can be doubt that almost every transvestite has various difficulties in his interpersonal relations requiring some kind of psychotherapy. The results of such treatment should not be overestimated, and its possibilities and limitations should be clearly realized. Nonetheless, it seems that in quite a few cases some encouraging results were achieved by a superficial psychotherapy, an understanding and permissive attitude, and some environmental manipulations (*e.g.* our Cases "A" and "D"). There are even reported cases of a complete cure through psychotherapy (London; Deutsch).

On the whole, psychotherapy seems to have some chance for success in mild and uncomplicated cases of genuine transvestism, provided the patient really desires to be cured. However, only very few transvestites have such desire. Almost as a rule, a transvestite merely "wants advice on how to keep working, without breaking down, and without getting into trouble" (Grotjahn). If he ever comes to a physician on his own initiative, it is usually with the sole aim of obtaining a certificate to the effect that the wearing of clothes of the opposite sex is indispensable for his mental health *(e.g.* our Case "C"). The only other occasion when a doctor may see a transvestite is when the latter, being prevented from cross-dressing, develops a psychoneurotic reaction that necessitates treatment (*e.g.* our patient "A").

Physical methods of treatment. Physical methods cannot cure transvestism as such, but can relieve the patient from some accompanying psychoneurotic symptoms, such as depression, feelings of guilt, anxiety *(cf.* our Case "A"), and thus enable him to cope more efficiently with his difficulties, and eventually make a more satisfactory adjustment. Thus in spite of their limitations, the physical methods of treatment (*e.g.* ECT, Modified Insulin, etc.), can be a valuable adjuvant in the therapy of transvestism.

There seems to be only one recorded instance where a physical treatment alone (ECT) brought the transvestite manifestations to an end. This case, however, is atypical inasmuch as the transvestite behavior was precipitated by a schizo-affective psychosis, and disappeared together with the psychotic episode itself (Liebman's case).

Glandular treatment. This procedure, which amounts to a "chemical castration", has only been applied to male transvestites (as administration of estrogen), with the aim of inhibiting the testicular function, and causing a secondary "feminization." There are few advocates of this "treatment". Benjamin recommends in milder cases of transvestism psychotherapy, that it be "supplemented by endocrine treatment at the earliest possible stage." Hamburger also advocates glandular treatment, though he uses it rather as a preparatory measure for the subsequent operative castration, and not *per se.*

Surgical "treatment". This is most controversial, as it involves ethical and legal problems as well as medical ones. The main reservation is the obvious irreversibility of a conversion-operation: should the "demasculinized" patient later change his mind, no surgeon could provide him with artificial testicles and an artificial penis, although the same surgeon might have created for him an artificial vagina. Such a situation is not only a theoretical possibility; it has already actually occurred. Patient "B" of Bättig, four years after his castration and "legalization as a woman," "came back as a man into the psychiatric policlinic ... He lost his illusions ... he cared now only to be 'changed' again into a man ... He was utterly lonely, very depressed ... blaming the authorities, but no less the doctors" (Bättig). This poor creature presented a most pathetic picture of a "cured" transsexualist, and is a grave warning to all enthusiasts of conversion-operation. Thus it is not surprising that Boss (16, 17) was severely criticized by Mitscherlich and other participants of a psychiatric congress, when he reported a case of a male transvestite, in whom (after all psychotherapeutic efforts failed, and the distressed patient threatened self-mutilation, or suicide), he recommended a conversion-operation.

Yet in spite of all this, and in spite of the fact that, "Sex, being genetically determined, cannot be changed" (Benjamin), there are still some authors who favor a "surgical treatment." Curiously enough, Benjamin himself recommends such procedure "in suitable cases" although it "is to be considered only as a last resort." Dukor also feels that in his third group of transvestites (with a "neurotic genesis") there is a true suffering, and that, after all other kinds of treatment proved unsuccessful, an operation should be "legally granted and justified from the point of view of medical ethics."

The main supporter of surgical treatment is Hamburger, who recommends the following measures: 1, "permission to wear women's clothes in public;" 2, "legal recognition and registration as a woman;" 3, "administration of estrogenic substances;" 4, "castration" (here follows a lengthy polemic on the lines, "With what right society compels persons to tolerate the presence of these organs?"); 5, "demasculinization" ("If operative castration has been carried out, there can be no serious objection to amputation of the penis and plastic surgery of the scrotum..."), and, finally; 6, "Formation of artificial vagina."

Hamburger's standpoint brought strong words of protest. "The ethical arguments of the authors... lead... to a somewhat disturbing *reductio ad absurdum.* If a patient has a wish to die, should the physician actively comply with the patient's wish?" (Ostow). "And what about a patient's demand that his eyes be removed because they commit 'sins,' or that his right arm be amputated to stop him from masturbating? Should medicine give in in a case of this type?" (Gutheil). Dreyfus warns that the question of "the change of sex... should receive much more reserve and consideration in cases where the source of desire is in morbid psychic imagination." Many other writers reject a conversion-operation as being "against nature" (Mitscherlich, Gollner, Mohr, and others).

In the opinion of the present writer surgical "treatment" is contrary to medical ethics, is without any rationale, and also is far from being free from a serious surgical risk. (Glaus quotes a case of a sudden death from a pulmonary embolus after a "successful" castration and ablation of penis on a transvestite.)

It is tempting to conclude this report by expressing the hope that in the foreseeable future the hypothetic "areas" of abnormal sexual discharge, postulated by Thompson (95), might be physiologically established and anatomically delineated. Then all the horrors of castration, peotomy, and "creation of an artificial vagina" would disappear, and be superseded by some surgical treatment of Thompson's cerebral "areas", perhaps, on lines analogous to leucotomy.

CONCLUSIONS

1. Transvestism is a sexual deviation, met in all ages, and in all cultures.

2. It is most often confined to a desire to cross-dress only; less often there is a wish to cross-dress and to be socially recognized as a "woman."

3. Transsexualism is characterized (apart from the desire for cross-dressing) by a morbid urge for an anatomical "change of sex."

4. If this urge is frustrated, it may precipitate severe psychoneurotic reactions, self-mutilation, and even suicide.

5. It is important not to confuse transvestism with other related sexual deviations (such as homosexuality, fetishism, masochism, exhibitionism, etc.) although the traits of some of these deviations may be found almost in every case of transvestism.

6. It is also important to adhere to the division of the phenomenon into two separate clinical syndromes, one of transvestism, and one of transsexualism. This facilitates the understanding of the problem, and its prognostic and therapeutic evaluation.

7. The course of transvestism is self-limiting, that of transsexualism incessant.

8. A male heterosexual transvestite is

usually capable of making a fairly satisfactory adjustment to marriage, provided he meets with an understanding and sympathetic attitude on the part of his wife. It seems that all female transvestites are homosexual, and unable to adjust themselves in a heterosexual marriage.

9. Genuine transvestism and transsexualism appear to be male sexual deviations exclusively brought about by a pathological female identification.

10. Cultural factors are of utmost importance in the development and the social status of transvestism.

11. Neither transvestism nor transsexualism are hereditary.

12. The attitude of the social environment toward a transvestite should be sympathetic and permissive, and psychotherapy may help him to achieve a compromise between his transvestite cravings and the social prohibitions. There is at present no satisfactory treatment for transsexualism.

SUMMARY

The history and geographic distribution of transvestism has been reviewed, the question of terminology discussed, definitions presented, and the classification of the phenomenon into one of "transvestism" and one of "transsexualism" suggested.

Several etiological theories of transvestism have been reviewed and discussed.

The clinical picture of transvestism and transsexualism has been sketched and illustrated by excerpts from recent literature. Transvestism has been divided into a milder form, confined mostly to cross-dressing only; and a more active form, with a profound feminine identification, involving claims of having "menstruation", dreams and day-dreams of pregnancy and motherhood. In this connection illustrative examples have been quoted.

With regard to its sexual aim, transvestism has been classified into the following groups. 1. A-sexual, 2. Auto-mono-sexual, 3. Hetero-sexual, 4. Homo-sexual, and 5. Bi-sexual. Their characteristics and frequency of occurrence have been discussed.

The relationship between transvestism and some other sexual deviations has been considered, and the following, more common, trends in transvestism elucidated. 1. Homosexual, 2. Fetishistic, 3. Narcissistic and Exhibitionistic, and 4. Masochistic. These conditions have been illustrated by cases from the current literature.

It has been suggested that the most characteristic feature of transsexualism is a morbid desire for a conversion-operation. A hypothesis has been put forward that all transsexualists are homosexual, and that transsexualism is a male sexual deviation.

Like transvestism, transsexualism has been clinically divided into mild and malignant forms. A third, intermediate type, has been illustrated by brief summaries of three cases.

The relationship between transvestism and transsexualism, and psychoneurosis and psychosis respectively has been discussed. The questions of heredity, age of onset, progress and outcome of both phenomena, frequency of occurrence, and sex distribution have been examined. The impact of transvestism on marriage, and the legal aspects of transvestism and transsexualism have been discussed. The important role of cultural and social factors on the development and the status of transvestism has been described.

Finally, various methods of treatment have been discussed and evaluated.

REFERENCES

1. Abraham, F. Genitale Umwandlung an zwei männlichen Transvestiten. Ztschr. f. Sexualwissensch., **18:** 233, 1931.

2. Abraham, K. Remarks on psychoanalysis of a case of foot and corset fetishism. In *Selected Papers*, Chap. 5. Hogarth Press and the Institute of Psycho-analysis, London, 1927.

3. Alexander, F. *Psychoanalysis of the Total Personality*. Nervous and Mental Disease Publishing Co., New York, 1930.

4. Aubert, G. *Trois cas de désir de changer de sexe*. Dissertation. Tavannes Imprimerie, Lausanne, 1947.

5. Bak, R. C. Fetishism. J. Am. Psychoanalyt. A., 1: 285–294, 1953.
6. Barahal, H. S. Female transvestism and homosexuality. Psychiat. Quart., **27**: 390–438, 1953.
7. Barr, M. L. and Hobbs, G. E. Chromosomal sex in transvestites. Lancet, **1**: 1109–1110, 1954.
8. Bättig, F. *Beitrag zur Frage des Transvestitismus.* Dissertation. Buchdruckerei Fluntern, Zürich, 1952.
9. Bender, L. and Paster, S. Homosexual trends in children. Am. J. Orthopsychiat., **11**: 730–743, 1941.
10. Benjamin, H. Transvestism and transsexualism. Internat. J. Sexol., **7**: 12, 1953.
11. Benjamin, H., Gutheil, E. A., Deutsch, D. and Shervin, R. V. Transvestism and transsexualism—a symposium. Am. J. Psychotherapy, **8**: 219–244, 1954.
12. Berman, L. Perception and object relation in a patient with transvestite tendencies. Internat. J. Psycho-Analysis, **34**: 24–39, 1953.
13. Binder, H. Das Verlangen nach Geschlechtsumwandlung. Ztschr. f. d. ges. Neurol. u. Psychiat., **143**: 84–174, 1932.
14. Bing, R. and Schönberg, S. Ueber Transvestitismus. Schweiz. med. Wchnschr., **52**: 1254–1258, 1922.
15. Bloch, I. *Das Sexualleben unserer Zeit.* L. Marcus, Berlin, 1919.
16. Boss, M. *Sinn und Gahalt der sexuellen Perversionen.* H. Huber, Bern, 1947.
17. Boss, M. *Psychopathologie der Liebesstörungen.* Bern, 1952.
18. Böhm, F. Bermerkungen über Transvestitismus. Internat. Ztschr. Psycho-Analysis, **9**: 497–509, 1923.
19. Bürger-Prinz, H. and Weigel, H. Ueber den Transvestitismus bei Männern. Monatsschr. Kriminalbiol. u. Strafrecht., **31**: 125, 1940.
20. Bürger-Prinz, H., Albrecht, H., and Giese, H. Zur Phänomenologie des Transvestitismus bei Männern. Beitr. Zexualforsch., V. 3. F. Enke Verlag, Stuttgart, 1953.
21. *Daily Express,* London, October 4, 1951.
22. *Daily Mail,* London, March 25, 1958.
23. *Daily Mail,* London, September 16, 1958.
24. Delay, J., Deniker, P., Lemperiere, T. and Benoit, J. C. Histoire d'un travesti: L' Eonisme. Encéphale, **5**: 385–398, 1954.
25. Deutsch, D. A case of transvestism. Am. J. Psychotherapy, **8**: 239–242, 1954.
26. Dreyfus, J. R. Der Geschlechtswechsel beim Hermaphroditismus. Schweiz. med. Wchnschr., **81**: 6–10, 1951.
27. Dukor, B. Probleme um den Transvestitismus. Schweiz. med. Wchnschr., **81**: 516–519, 1951.
28. East, N. *Society and the Criminal.* His Majesty's Stationery Office, London, 1949.
29. Ellis, H. Eonism and other studies. In *Studies in the Psychology of Sex,* Vol. 2. Random House, New York, 1936.
30. Fenichel, O. The psychology of transvestism. Internat. J. Psycho-Analysis, **11**: 211–227, 1930.
31. Fenichel, O. The symbolic equation: Girl = phallus. Psychoanalyt. Quart., **18**: 303–324, 1949.
32. Fenichel, O. *The Psychoanalytic Theory of Neurosis.* W. W. Norton & Co., New York, 1945.
33. Fessler, L. Ein Fall von posttraumatischen Transvestitismus. Arch f. Psychiat., **100**: 232–252, 1933.
34. Ford, C. S. and Beach, F. A. *Patterns of Sexual Behavior.* Harper, New York, 1951.
35. Frazer, Sir J. G. *Magic and Religion.* Watts & Co., London, 1945.
36. Freud, S. Fetishism. Internat. J. Psycho-Analysis, **9**: 161–166, 1928.
37. Freud, S. *Three Contributions to the Theory of Sex.* Nervous and Mental Disease Publishing Co., New York, 1910.
38. Freud, S. Splitting of the ego in the defensive process. In *Collected Papers,* Vol. 5, pp. 372–375. Hogarth Press and the Institute of Psycho-Analysis, London, 1950.
39. Friend, M. R., Schiddel, L., Klein, B. and Dunaeff, D. Observations on the development of transvestism in boys. Am. J. Orthopsychiat., **24**: 563–575, 1954.
40. Gillespie, W. H. Notes on the analysis of sexual perversions. Internat. J. Psycho-Analysis, **33**: 397–402, 1952.
41. Glaus, A. Zur Lebensgeschichte eines Transvestiten ... Monatsschr. Psychiat. u. Neurol., **124**: 245–258, 1952.
42. Glaus, A. Sexuelle Störungen und Militärdienst. Vjschr. Schweiz. Sanit. Off., **9**: 1951.
43. Goldschmidt, R. *Die sexuellen Zwischenstufen.* J. Springer, Berlin, 1931.
44. Greenacre, P. Certain relationships between fetishism and the faulty development of the body image. Psychoanalyt. Study of the Child, **8**: 79–98, 1953.
45. Grotjahn, M. Transvestite fantasy expressed in a drawing. Psychoanalyt. Quart., **17**: 340–345, 1948.
46. Guiraud, P. Délire systématisé et inversion sexuelle. Ann. méd. psychol., **12** series, **2**: 128–132, 1922.
47. Gutheil, E. Analysis of a case of transvestism. In Stekel, W. *Sexual Aberrations,* Vol. 2, pp. 281–318. Liveright, New York, 1940.
48. Gutheil, E. The psychological background of transsexualism and transvestism. Am. J. Psychotherapy, **8**: 231–239, 1954.
49. Hamburger, C., Stürup, G. K. and Dahl-Iversen, E. Transvestitism; hormonal, psychiatric and surgical therapy of case. Nord. med., **49**: 844–848, 1953.

50. Hamburger, C. The desire for change of sex as shown by personal letters from 465 men and women. Acta endocrinol., **14:** 361–375, 1953.

51. Hammond, W. H. Am. J. Neurol. & Psychiat., August, 1882 (after Ellis).

52. Hirschfeld, M. *Verkleidungstrieb mit umfangreichen casuistischen und historischen Material.* Max Spohr, Leipzig, 1910.

53. Hirschfeld, M. *Sexual Anomalies and Perversions.* F. Alder, London, 1936.

54. Honigman, J. J. Cultural dynamics of sex. Psychiatry, **10:** 37–47, 1947.

55. Horton, C. and Clarke, E. K. Transvestism or eonism. Am. J. Psychiat. **10:** 1025–1030, 1931.

56. James, R. E. Precipitating factors in acute homosexual panic (Kempf's Disease) with a case presentation. Quart. Rev. Psychiat. & Neurol., **2:** 530–533, 1947.

57. Jelgersma, A. A peculiar custom in the Island of Maarken. Internat. J. Psycho-Analysis, **6:** 288–291, 1925.

58. Karpman, B. Dream life in a case of transvestism. J. Nerv. & Ment. Dis., **106:** 292–337, 1947.

59. Karpman, B. Mediate psychotherapy and acute homosexual panic (Kempf's Disease). J. Nerv. & Ment. Dis., **98:** 493–506, 1943.

60. Kempf, E. *Psychopathology.* C. V. Mosby, St. Louis, 1920.

61. Kinsey, A. C. *Sexual Behavior in the Human Female.* Saunders, Philadelphia, 1953.

62. Klaproth. *Reise in den Kaukasus.* Berlin, 1812 (after Krafft-Ebing).

63. Klein, M. *Psycho-Analysis of Children.* Hogarth Press and the Institute of Psycho-Analysis, London, 1932.

64. Krafft-Ebing, R. von. *Psychopathia sexualis.* Pioneer Publications, New York, 1947.

65. Lang, T. Genetic determination of homosexuality. J. Nerv. & Ment. Dis., **92:** 55–64, 1940.

66. Liebman, S. Homosexuality, transvestism and psychosis. J. Nerv. & Ment. Dis., **99:** 945–958, 1944.

67. London, L. S. *Mental Therapy; Studies in Fifty Cases.* Covici-Friede, New York, 1937.

68. Lukianowicz, N. Two cases of transvestism. (soon to appear).

69. Lukianowicz, N. Transvestism and psychosis. (soon to appear).

70. Masson, A. *Le Travestissement.* Dissertation. Le François, Paris, 1935.

71. Mayer-Gross, W., Slater, E. and Roth, M. *Clinical Psychiatry.* Cassel, London, 1954.

72. Mead, M. *Sex and Temperament in Three Primitive Societies.* W. Morrow, New York, 1935.

73. Mead, M. *Male and Female.* Victor Gollancz, London, 1949.

74. Moll, A. *Handbuch der Sexualwissenschaften.* C. Vogel, Leipzig, 1921.

75. Moll, A. *The Sexual Life of the Child.* G. Allen & Unwin, London, 1923.

76. Moore, K. L. and Barr, M. L. Morphology of nerve cell nucleus in mammals with special reference to sex chromatin. J. Comp. Neurol., **98:** 213–231, 1953.

77. Moore, K. L., Graham, M. A. and Barr, M. L. Detection of chromosomal sex in hermaphrodites from skin biopsy. Surg. Gynec. & Obst., **96:** 641–648, 1953.

78. Moszkowicz, L. Intersexualitätslehre und Hermaphroditismus und ihre Bedeutung für die Klinik. Klin. Wchnschr., **8:** 289–337, 1929.

79. Oemig, O. Beitrag zur Lehre vom Transvestitismus. Ztschr. ges. Neurol. u. Psychiat., **15:** 190, 1913.

80. Olkon, D. M. and Sherman, I. Eonism with added outstanding psychopathic features. J. Nerv. & Ment. Dis., **99:** 159–167, 1944.

81. Ostow, M. Transvestism. J.A.M.A., **152:** 1553, 1953.

82. Peabody, G. A., Rowe, A. T. and Wall, J. M. Fetishism and transvestism. J. Nerv. & Ment. Dis., **118:** 339–350, 1953.

83. Petritzer, B. K. and Foster, J. A case study of a male transvestite with epilepsy and juvenile diabetes. J. Nerv. & Ment. Dis., **121:** 557–567, 1955.

84. Pettow, Z. Zur Psychologie der Transvestie. Arch. ges. Psychol., **22:** 221, 1912.

85. *Picture Post,* London, March 13 & 20, 1954.

86. Podolsky, E. *Encyclopedia of Aberrations.* Arco Publications, Ltd., London, 1953.

87. Redmount, R. S. A case of a female transvestite with marital and criminal complications. J. Clin. & Exper. Psychopath., **14:** 95–111, 1953.

88. Rosanoff, A. J. A theory of chaotic sexuality. Am. J. Psychiat., **91:** 35–41, 1935.

89. Sadger, J. *Die Lehre von Geschlechtsverirrungen.* Vienna, 1921.

90. Shankel, L. W. and Carr, A. C. Transvestism and hanging episodes in a male adolescent. Psychiat. Quart., **30:** 478–493, 1956.

91. Shervin, R. V. The legal problem in transvestism. Am. J. Psychotherapy, **8:** 243–244, 1954.

92. Stekel, W. *Der Fetishismus.* Berlin, 1923.

93. Tennenbaum, J. *The Riddle of Sex.* G. Rutledge & Sons, Ltd., London, 1930.

94. *The Justice of the Peace,* February 26, 1938.

95. Thompson, G. N. Electroshock and other therapeutic considerations in sexual psychopathy. J. Nerv. & Ment. Dis., **109:** 531–539, 1949.

96. "Thompson, Janett" Transvestism; an empirical study. Internat. J. Sexol., **4:** 216–219, 1951.

97. Thürlimann, R. *Ueber die Indikation und den therapeutischen Erfolg der Kastration bei sexuell Perversen.* Dissertation. Art. Inst. Orell Füssli, A. G., Zürich, 1945.

98. **Waggoner, R. W. and Boyd, D. A., Jr.** Juvenile aberrant sexual behavior. Am. J. Orthopsychiat., **11:** 275–291, 1941.

99. **Westphal, C.** Die conträre Sexualempfindung. Arch. Psychiat. u. Neurol., **6:** 73, 1876.

100. **Wiedeman, G. H.** Transvestism. J. A. M. A., **152:** 1167, 1953.

101. **Wilson, G. W.** A study of structural and instinctual conflicts in cases of hay fever. Psychosom. Med., **3:** 51–56, 1941.

102. **Wilson, G. W.** A further contribution to the study of olfactory repression with particular reference to transvestism. Psychoanalyt. Quart., **17:** 322–339, 1948.

103. **Worden, F. G. and Marsh, J. T.** Psychological factors in men seeking sex transformation; preliminary report. J. A. M. A., **157:** 1292–1298, 1955.

104. **Wyrsch, J.** Selfstverstümmelung eines Transvestiten. Schweiz. med. Wchnschr., **74:** 657, 1944.

SOCIAL FORCES AND THE NEUROTIC PROCESS

LAWRENCE S. KUBIE, M.D.[1]

INTRODUCTION

There is a widespread conviction that a close interdependence exists between individual psychopathology and the pathology of social structure and of cultural forces. If this is true, it is of utmost importance for the future of the human race as a whole, for human society, and for each individual human being. For this very reason, however, it is essential not to oversimplify the nature of their interdependence, nor to delude ourselves with the many easy cliches and deceptive fallacies with which this subject is infested.

Citing some frequent simplifications will give point to this. Each statement that follows will be an exaggeration; yet in the literature each recurs in many disguises, often hedged around with qualifications which seem to bring it closer to the truth, without eliminating the essential error.

1. "Neuroses are due to poverty." A more careful statement is that without poverty you would have no, or fewer, or less severe, or different kinds of neuroses, or the still more careful formulation by Leavy and Freedman (9).

2. "Neuroses are due to wealth and leisure." Again stated more carefully, it is the surfeit of wealth, the inability to use leisure, the lack of anything to do, the lack of occupation, the lack of work, idleness, which produce neuroses or intensify them. It is not irrelevant that in quick succession the same individual will defend both of these opposing positions without realizing their irreconcilability.

3. Neuroses are due either to overwork, (or else to underwork) to excessive fatigue (or to excessive indolence).

4. Again, we find neuroses attributed to ignorance and/or to excessive education, to being an "egghead," to superstition, or to super-sophistication.

5. Neuroses are attributed to lack of love, or to an excess of loving.

6. Neuroses are due to neglect; or, obversely, to overprotection.

7. Still others blame neuroses on the simplicity, the barrenness and the isolation of country life; or conversely on the pace, the congestion and the complexities of urban existence.

8. Primitivism in all its forms is blamed; but against this is balanced the attribution of the neurotic process to the highly rarified and precious air of the most cultivated and "civilized" modern existence.

9. Neurosis is blamed by some on superstition and credulity; but also on an overcompliant, passive acceptance of religious faith. Conversely neurosis is blamed on irreligion and skepticism.

10. Neurosis is blamed on puritanism and moralistic taboos, or conversely on license, instinctual self-indulgence and vice.

11. There was a period, not long past, when neuroses were blamed on in-bred family life. For instance the novels of Louis Couperus described the fetid air of neurosis in the intricate network of family life in Amsterdam. Swiss writers were painting similar pictures of Basel. Others were doing the same for Jewish families, both for the red damask era of established American Jewry, for the Jewish immigrant, for the Anglicized Jew in Britain (Louis Golding,

---

[1] Department of Psychiatry, Yale University School of Medicine, New Haven; Department of Psychiatry, College of Physicians and Surgeons of Columbia University, New York. This article is based on a chapter of the same title contributed by Dr. Kubie to the volume: Leighton, A., Clausen, J. and Wilson, R., Eds. *Explorations in Social Psychiatry*. Basic Books, Inc., New York, 1957.

G. B. Stern and others), and for the continental Jew (Wasserman, Feuchtwanger, etc.).

These stressed the secret accumulation of inexpressible patricidal, matricidal and fratricidal tensions in close-knit families, and the parallel pressures of equally inexpressible incestuous needs, as the soil out of which neurosis is born. Today, however, because of the current emphasis on domestic turbulence, superficial observers seem to forget these recent arguments, and place the blame almost exclusively on the breakdown and attenuation of family life, as a major source of the neurotic process, rather than viewing this as one of its important current manifestations.

Another evidence of this easy misinterpretation of the influence of family disruption is the fact that it overlooks the statistics of the situation. In a recent article (5) the writer pointed out that the statistics gathered by Paul H. Jacobson of the Metropolitan Life Insurance Company and others indicate that although the disruption rate due to divorce had risen in the years between 1890 and 1940, the disruption rate due to early deaths had dropped even more. The net result is that more families were holding together in 1940 than in 1890. Family disruption is not a new phenomenon. The reshuffling of marriages because of early deaths does not necessarily have the same impact upon youngsters as does a reshuffling through divorce; but there has been no objective comparative study of the effects of the two types of family disruption on the development of children.

It does not clarify the basic issue to acknowledge that every extreme position contains a measure of truth. Nor does it reconcile their inconsistencies to point out that in each instance there are "feed-back" relations, in that the condition which may be an outcome of the neurotic process (*e.g.*, family disruption) can also influence it in turn. In the interplay of psychosocial forces, cause and effect relationships are always circular. Yet the basic question remains unanswered; to wit, what initiates the neurotic process in infancy, and what role if any do social forces exercise in its inception? These questions must be clearly differentiated from questions concerning the role of social forces in the secondary molding and shaping of the neurotic process, and in determining the price paid by an individual, his family, and the community for his neurosis. All of these issues will be explored more fully below, after a description of the nature of the neurotic process itself.

THE NATURE OF THE NEUROTIC PROCESS

Probably nothing has been less conclusively defined than the nature of the neurotic process; and about nothing is there more confusion between laymen and behavioral scientists, among the several varieties of behavioral scientists, and even within the close fraternity of psychiatrists and the even closer fraternity of analysts. Therefore no definition can be looked upon as one to which all will agree. All that I have any right to hope is that if I express my own convictions clearly about the nature of the neurotic process, then for the purposes of my argument the reader may understand what I personally mean. If at the same time I also convince even a few readers that my definition goes to the heart of the matter, then my argument will in addition have a wider usefulness.

In the explication that follows, it will be clarifying to rule out the current popular and quasi-scientific misuses of the concept of neurosis which contribute to the universal confusion.

In the first place I will not use the words *neurotic process* to characterize any fully developed illness, although the neurotic *state* as an illness evolves out of a preexisting neurotic *process*, which may have been well hidden (4). Nor will I apply the words to any behavior, merely because that behavior is strange, unusual or infrequent, asocial, unconventional, and non-conformist in contrast to that which is statistically frequent, socially acceptable and approved, and conforming to the mores of any particular culture. The term will not be used as a synonym for behavior which is socially destructive as contrasted with socially creative and valuable. Statistical frequency has nothing to do with health. Common colds are illnesses no matter how many people suffer from them. Conformity and rebelliousness may equally be sick or well, normal or neurotic, depending upon whether they are voluntary or automatic; which, in turn, depends upon their determining circumstances, as will be explained below. Furthermore, there are many more neurotic do-gooders than do-badders, or the world would be in an even more parlous state than already exists. Nor will the words have to do with the strangeness or eccentricity or peculiar unfamiliarity or flamboyance of any pattern of behavior, since many manifestations of the neurotic process are masked behind a meticulously obscure, mediocre, and self-effacing facade.

Perhaps most important of all is the fact that I will not be talking about neurotic personalities, or neurotic constitutions, or neurotic cultures. A great deal is written and claimed about this in the technical literature of psychoanalysis and psychiatry, in the literature of cultural anthropology and social psychology, in the law and in the lay press. Yet our data are not sufficiently clear to make it possible to apply these terms with accuracy either to whole personalities or to whole cultures. Furthermore, without a broad area of meaning which is common to all, we cannot even be sure that we are communicating about the same phenomena. My conviction therefore is that for the present we must limit ourselves to trying to understand what determines whether any given moment of behavior is "normal" or "neurotic," or an admixture of both, as is the rule.

To clarify this I will venture to repeat an already published hypothesis, which utilizes the dynamics of behavior as the basis for a crude pragmatic test: (6) "Thus the essence of normality is flexibility, in contrast to the freezing of behavior into patterns of unalterability which characterize every manifestation of the neurotic process, whether in impulses, acts, thoughts, or feelings. Whether or not a behavioral event is free to change, depends not upon the quality of the act itself but upon the nature of the constellation of forces that has produced it. No moment of behavior can be looked upon as neurotic unless the processes that have set it in motion also predetermine its automatic repetition, irrespective of the situation, the utility, or the consequences of the act."

Naturally the question that will immediately arise is what is the nature of that peculiar constellation of forces which can predetermine a tendency to the automatic repetition of any act? The answer to this requires a few additional words of technical explanation. We view every moment of behavior as being the product of three groups of concurrent psychological forces: a group which operates on the *conscious* level, another group which operates on what is called the *preconscious* level, a third group that influences behavior from behind an iron curtain which can be penetrated only by special exploratory devices, to wit, the *unconscious* level. Wherever unconscious forces play the preponderant role in this constellation, dominating over the other two, then for a variety of reasons the behavior which results from the interplay among these three groups of forces is subject to a tendency to automatic and obligatory repetition. In part this is because the unconscious conflicts and purposes are represented by symbolic patterns of activity which can never reach their objectives. Furthermore, because their dominant determinants are unconscious these action patterns are not responsive to conscious experiences of pleasure or pain, success or failure, reward or punishment, argument or exhortation. Consequently, they are insatiable, automatic, rigid and incapable of learning from experience of any kind. Moreover this is true whether the forces are expressed through frank neurotic symptoms, through art forms, through creative acts in science, or through those subtle deformations of general patterns of behavior which together constitute what we call the personality.

In contrast to all of this, wherever conscious and preconscious forces are in the saddle, the resulting patterns of activity are alterable by experience. They can achieve their goals: therefore they can reach a state of comfortable satiety. Or when their goal proves to be unattainable, they can learn through the corrective experience of defeat to desist or change. Therefore, behavior which is determined by a constellation of processes in which conscious and preconscious forces predominate is alterable by the impact of success and failure, rewards and punishments, pleasure and pain, argument, logic and exhortation. It is a further consequence that such behavior is oriented to reality, and remains freely flexible. It is adaptive. It can learn. This is the essential difference between a normal and a neurotic act; between the normal and the neurotic process. It is the difference between eating to satisfy hunger and the compulsive eating of the neurotic child who eats until he vomits and then must eat again. I must repeat that the difference lies *not* in the act but in the constellation of forces which determine the act, and which predetermine at the same time its automatic repetition.

If we generalize from this to a concept of mental health in a broader sense, it is fair to say that greater health is achieved whenever important areas of life can be brought under the domination of conscious and preconscious processes (never exclusively, but to a major degree). Therefore the goal of therapy is to shrink those areas of life which are dominated preponderantly by the inaccessible unconscious processes. This is a wholly reasonable concept of normality, which leaves ample room for all of the vital and creative economies of preconscious functioning, and also for those unconscious processes which are not in irreconcilable conflict with conscious and preconscious goals.

At this point I should reemphasize the fact that this concept of the neurotic process is far removed from the usual derogatory connotations of the term. In the first place it starts from the fact that there is a neurotic potential in human nature which is universal (4, 6), because it is inherent in our psychological development, depending as it does upon the special role which symbolic functions play in human development. It is the power of symbolic thinking and feeling which endows Man with his unique creative potential; and it is also the vulnerability of these symbolic functions to distortions which gives rise to that aspect of the neurotic process which is peculiar to the human being. It is the symbolic process plus its vulnerability which gives rise to the neurotic potential.

Furthermore, out of this universal neurotic potential a neurotic process evolves in some degree in everyone. It varies in form and direction and intensity; but in some measure it plays a basic role in every life, and in every personality, including those lives which we think of as being approximately normal. Finally the point of view presented here implies that out of this variable but ubiquitous neurotic process, under appropriate circumstances, there precipitates something called "the neurotic state." Actually this neurotic state (or "neurosis") occurs universally as transient episodes in childhood. In some children these transient neurotic episodes gradually merge to become the more enduring neuroses of adolescence and of adult life. In others, the transient neurotic states of childhood disappear below the surface. Rarely if ever are they basically resolved. Usually they leave the individual scarred. For years he may remain free from crystallized neurotic illness; but in time the cumulative effects of these scars catch up with him, and the neurotic process then "decompensates" with the eruption of the full-blown neurosis or psychosis of later life. The influence of those neurotic episodes which seem to disappear spontaneously actually persists as a process which may warp and color an entire life. Consequently in the adult population the so-called normal is usually an individual who does not have a symptomatic neurosis, but in whom the neurotic process plays a more subtly concealed role, influencing the make-up of his personality, his life adjustments, his work and play, his loves and hates, and his health in general. The so-called "neurotic" among adults is merely one in whom, there are in addition, those crystallized neurotic states which we call the psychoneuroses. These are self-diagnosing. Their recognition requires no skill or subtlety. Indeed such patients come to us without urging, telling us that they have fears, depressions, compulsions, obsessions, etc. With few exceptions they seek help on their own initiative for the relief of their painful symptoms. Yet the more subtly concealed neurotic process, which may be devoid of overt neurotic symptoms, is often far more destructive of human health and happiness. The concealed neurotic mechanisms are so interwoven into the fabric of the personality that most human beings defend to the death their right to retain their prized neurotic quirks.

Here is where culture exercises a profound influence (Redlich, Hollingshead, *et al.* (2, 3)). The cultural tolerance for overt neurotic symptoms may vary widely in different settings, as well as cultural tolerance for those personality quirks which manifest in concealed ways the subtle influence of the neurotic process. Therefore culture is one of the forces which determine if and when a patient will look upon himself as ill or at least as needing help, and whether he will seek help. Ultimately, however, what determines a patient's attitude toward his own neurotic process and neurotic symptoms is the amount of pain these cause him. If he lives long enough everyone in the end pays a high price for his own neurotic process; but as will be evident in some of the illustrations to be given below, the pain of a neurotic symptom may be urgent, insistent and immediate in its demand for help; whereas for many years the neurotic process may cause pain to others and not to the patient himself. Consequently he may not seek or accept treatment until late in life. Many a successful man comes to grief in the later decades because the neurotic process with which he has sparred successfully for years, and which he has even used successfully, finally catches up with him. Many a campus hero comes for treatment in the 30's and 40's, only to reveal the fact that underneath the surface of a seemingly smooth adjustment in school and college, neurotic mechanisms have been at work, which until that moment he had been able to utilize and exploit. Indeed the universal but masked neurotic ingredient in all human nature is the ultimate basis of the human discontent which occurs in even the most successful and the most privileged among us.

There is one other point that I should clarify here, because it concerns the nature of the neurotic process and its relationship to the psychosis. This again is an issue about which there is both uncertainty and disagreement. Nor is it possible to go into it in detail without over-extending this paper. I can say only that apart from those psychoses which are caused by organic disruption of nervous structure and function, the psychosis arises when an acute failure occurs in the defenses against a neurotic process. Sometimes this occurs in early years (7); sometimes only after the defenses have worked satisfactorily for years. Either way, when the break occurs it results in chaotic disorganization of all psychological processes. The resulting state of psychosis has many special psychological characteristics and symptomatic manifestations; but I do not believe that it is necessary or relevant to detail these in this connection.

With this triad in mind (*i.e.,* a universal neurotic potential which is inherent in human nature; a highly variable but ubiquitous neurotic process which evolves out of this universal potential; and a neurotic state which under appropriate circumstances precipitates out of the neurotic process), our next problem becomes the question of what forces determine the inconstant evolution of this neurotic process.

Many variables determine this: *e.g.,* physiological, cultural, and social forces, individual idiosyncratic events, catastrophic early experiences, and early but continuous stresses. All of these influence the development of our psychological processes and their symbolic representations on all three levels or systems: to wit, conscious, preconscious and unconscious. At this point I can anticipate what is to follow by saying that cultural influences play an obvious role in those imitative steps which determine the acquisition of conscious psychological processes. They play a more subtle role in all preconscious imitative and repetitive processes, which are inherent in the learning process; and in which, at the same time, individual emotional experiences also play an important role. When, however, we ask what role cultural influences play in the splitting off of unconscious processes from the main stream of conscious and preconscious function (which is the quintessence of the neurotic process) we face an issue which is not yet understood even vaguely.

It is easy to characterize this splitting off of the representatives of inner events so as to render them unconscious, as the operation of a mechanism called "Repression." But the mere naming of it does not explain it. Nor has the role of cultural forces in repressive processes been explored; although it is precisely here that the impact of highly charged individual experiences plays a major role. It would be hazardous to claim that cultural forces, operating in subtle forms, exercised no influence on the repressive processes. But one can say with certainty only that the precise interaction remains obscure, and that it is related to many other unsolved issues, as for instance the role of heredity both in cultural patterning in general and in repression in particular; *i.e.,* whether or not the experience of the race becomes incorporated in our nervous system in such a way as to determine whether certain impulses and conflicts will be experienced consciously or unconsciously, and whether in turn these impulses are transmitted by hereditary forces and not by imitation. If racial history exercises such an effect, then we must also conclude that there are unconscious processes which have never been either preconscious or conscious, and which consequently have never been subjected to the processes of repression. It is experimentally proven that some experiences can be preconsciously perceived and buried in the unconscious without ever having been consciously experienced. The precise relation of this process to neurotogenic repression is still unclear. About these critical issues there is heated disagreement but no conclusive evidence; and until these basic alternatives have been subjected to critical experimental investigation all formulations about the impact of culture must be made quite tentatively.

THE ROOTS, TRUNK AND BRANCHES OF THE NEUROTIC PROCESS

It will be helpful and clarifying to bear in mind the many banal experiences which should make us ask ourselves whether we are dealing with the roots of the neurotic process or with those many later forces which shape and alter its subsequent evolution, secondary forces which make an enormous difference in the price which the individual pays for his neurosis but not in its genesis. It is conceivable that environmental forces may alter both the matured forms of the neurotic process, and its individual and social consequences, without necessarily playing a significant role in its genesis.

All of us can think of examples which demonstrate that the relationship between the roots of the neurotic process and cultural forces is subtle and elusive, and not to be dismissed by the easy generalizations of which I gave a few examples in the introductory section.

During the war soldiers whether in training camp or in combat would develop neurotic symptoms or somatic disturbances, the form of which was influenced by their national and cultural backgrounds whenever a group from a common origin chanced to be placed in the same unit for a considerable period. Here the influence of culture was not on the incidence or primary origins of neurosis but on its form. Similarly, the price one will pay for an agoraphobia or a claustrophobia will be quite different if one lives in a New York City tenement or on an Australian sheep ranch; and the price one pays for a height phobia will be different in the Rockies or on the Great Plains. In one situation one may be symptom-free and in another haunted by terror.

Again, we should not overlook the fact that society rewards some neuroses even as it punishes others. Thus the consequences to society, to the family of the patient, and to the patient himself are quite different for complusive benevolence or compulsive greed. I cite these only to underscore the fact that the interaction between social forces and the neurosis is easily traced in terms of the secondary and tertiary consequences of the neurotic process. The difficult issue concerns the relationship of culture to the origins of the neurotic process.

I have frequently cited a group of Scotch-Canadian woodsmen, who enjoyed economic and physical security, lived a physically challenging and toughening existence, in comfortable seclusion and yet with ready access to others. During a period of seven weeks in which I lived in the midst of two such families, I was consulted professionally by every member of both families. Not only was it evident that the neurotic process was universal in the group; but even more impressive was the fact that the quality of their neuroses was indistinguishable from that which challenged me daily in my private office, and in the psychiatric out-patient clinic of a general hospital in New York, the origins of whose patients were predominantly Italian, Jewish and Latin-American.

I could cite the example of a family in which a simple event occurred to a series of four children whose ages were nine, seven, five and two-and-one-half years. This event made a significant impact on the psychopathological development of each child; the difference depending on the age of the child, its sex, and on the ordinal position of each child in the group. The culture was constant. The neurotic process was as varied as four lives could be.

I recall the children in a family of sturdy, loving, "healthy" people, where the father was somewhat compulsively disorderly, happy-go-lucky and irresponsible in the details of existence; whereas the mother was compulsively orderly, meticulously clean, and with a compensatory responsibility in all daily and financial routines. The "neurosis" of each parent was expressed in a form which was easily acceptable, falling well within the social norm. In the eyes of the world each made a happy social and familial adjustment. Only the fact that professional training brought each parent into analysis, brought to light these well-masked, well-compensated neuroses. In no statistical table would this have been called a family dominated by neurotic parents. Yet it was not the culture but the subtle interplay between these two "normally" neurotic yet conflicting personalities, plus the ordinal positions, age intervals, and sexual sequence among the children, which were the critical forces which determined the evolution of each of the children and the shape of the neuroses which developed in each.

I can compare a number of sets of identical twins, who were brought up with the closest possible mutual identification, treated almost as two halves of a single human being, yet who showed remarkable divergencies in the patterns of the neuroses which evolved. In one instance two little girls were the last in a family of fourteen. One grew up with an unconscious identification with the father's aggressive, eloquent and boisterous temperament, an identification which colored every detail of her life until her death at the age of 78. The other evolved an equally strong but purely consolatory identification with a gloomy, defensive, conscience-ridden, sourly benevolent mother. This colored her behavior until her death at 75.

Again, early in life a set of monozygotic male twins diverged to such a degree that one developed an obsessional-compulsive work drive and the other an obsessional block both in work and play. The initiating steps which had preceded this bifurcation in the two neurotic processes had been identical; yet under the later influence of deep unconscious rivalry they split to follow divergent paths. The subsequent evolutions of the two neuroses were as different as day and night. One went on to a career of external achievement and success, which for many decades won him social prestige, economic advantages and considerable academic distinction as the rewards for his neurosis; until finally in the fifties his neurosis decompensated into an involutional depression. The other twin led a life of incessant pain, frustration and humiliation which by his late teens had driven him into a chronic hypochondriacal depression. This gradually acquired secondary paranoid features which colored all his years as he moved slowly to his death.

In another family group the father and mother married for unconscious reasons, which expressed another set of well-masked and evenly balanced neurotic forces. Unwittingly the father had married the mother as a heterosexual mask for his unconscious homosexual attachment to her brothers. Equally unwittingly the mother had married the father as a mask for her incestuous attachment to the same brothers, and also as an effort to salve the deep hurts which their rejection of her had always caused. Out of this painful struggle came children, every detail of whose orientation in life bore the imprint of the interplay between these two "sick" parents, both of whom were making exemplary social adjustments: the husband as a lawyer, the wife as a research scientist.

These are types of stresses which play an obvious role in the secondary evolution and shaping of the neurotic process. What role they play in its initial origins is by no means so clear. The answer to this question demands such microscopic examination as is accessible almost exclusively in analysis. Yet even in analysis such basic data are obtainable only under the most favorable and controlled and cross-checked circumstances. For such studies we need special research institutes. For many reasons, private practice *alone* does not yield dependable data of this type and depth. Rarely if ever are such data accessible to cultural anthropological scrutiny. Yet without it, one is unable to evaluate accurately the relative influence of the more easily accessible external social forces. Without such data in depth it is all too easy either to overemphasize the role played by the surface forces of society, or to dismiss them as inconsequential.

UNIVERSALS IN THE EVOLUTION OF THE HUMAN NEUROTIC PROCESS

Such considerations as these reinforce my conviction that we must begin our efforts to understand the role of social forces in the neurotic process by seeking to characterize the neurotogenic universals: *i.e.,* those forceful processes which, as far as we now know, exist in every human life under every culture known to man, and which are repeated in every existence. These universals are the baseline. Once they are determined, we will be in a position to evaluate the influence of variables introduced by different cultural, economic, social, educational and religious patterns. But until we understand the universals and their role in neurotogenesis, it will remain impossible to evaluate accurately the influence of these other variables.

If we allow ourselves to assume that we have solved the many technical problems of adequate representative sampling, of communication, of long-term observations, of cross-checking, recording and evaluating, and if we assume that we are at last competent to evaluate both the universal and the variable social and cultural forces, what then will be the critical questions which we would have to ask ourselves about their impact? Our answers to this will depend upon what we consider to be the essential ingredients in the neurotic process. I will not presume here to try to give an exhaustive catalogue of these. I will mention only those which at the moment seem to me to be of primary importance.

1. In the pre-verbal and largely presymbolic phase of infancy, I would place primary emphasis on a type of experience the importance of which Spitz (10) has demonstrated irrefutably: namely, the preverbal security, warmth, stillness, comfort, monotony, peace, closeness, ease and familiarity, which is engendered by being surrounded by familiar inanimate and animate objects. The first epitome of this is the mother's face and the interwoven visual, oral and olfactory experiences of the mother's body (the constellation of breast, bottle, and supporting arms), plus the incessant slow play of familiar sensory stimulation from handling, cleansing, feeding, cooing, rocking, carrying and playing. Into this infantile nirvana of constancy comes the primary disruptive impact of separation, pain, startle, neglect, starvation. The duration of that nirvana, its quality, constancy and its consistency, when and how it is disrupted (*i.e.,* whether gradually, intermittently, or suddenly), what constant substitutes are provided if any; all of this makes the first profound and lasting imprint on the emotional organization of that infant.

I would link closely to these, a type of experience which follows soon after: namely, the long years, which to every human child seem endless, in which he lives as one helpless Lilliput among hordes of brutal rival Lilliputs in a Brobdingnagian world of giants who are always giving either too much protection or too little. (How different cultures handle these stressful banal realities of every life has many consequences.)

2. It seems probable that these basic early experiences accumulate to establish in each individual a central emotional position, to which the personality returns automatically from any excursion to other emotional states. This central position can be pleasant and comforting, in which case the individual spends his life seeking it, protecting it, preserving it and attempting to recreate and recapture it, by every conscious, preconscious and unconscious device at his disposal. Alternatively, the central emotional position may be one of pain (*i.e.,* fear, depression, rage), against which the individual spends his life defending himself by conscious, preconscious and unconscious devices, whose aim it is to avoid this pain-filled central emotional position. I must repeat that these defensive maneuvers, like those which seek to regain happy central emotional positions which have been lost, are carried out unconsciously, preconsciously and consciously, in varying proportions. Among these it is the unconscious maneuvers, whether designed to regain and protect a happy state or to avoid a painful

state, which play a vital role in neurotogenesis. (Parenthetically, I would point out that we do not know today to what extent these patterns are alterable merely by gaining analytic insight into them.) The important point is that, whether we are protecting and seeking a pleasant central position or defending against a painful one, whenever such maneuvers are preponderantly unconscious they enter into the structure of the universal masked neurotic component which is to be found in all personalities. This is not true where the same maneuvers are carried on consciously and/or preconsciously.

These considerations pin-point for us two ways in which cultural forces may influence the origins of the neurotic process. In the first place, economic and social forces play a significant role in the parent-child environment in general, and in encouraging or limiting sudden changes and disruptive separations in the infant world. The young widow who is forced to go out to work, or the ambitious young woman who wishes to be both a mother and a career woman, or the frivolous young mother who leaves her infant for long hours out of every day to shop and play golf or cards, are equally the instruments of separations which will help to create the central emotional position which will shape a human life throughout its existence. Nor does it matter to the infant or child whether the mother is absent playing cards or scrubbing floors. Indeed, the pitiful paradox is that the frivolous card-playing absentee who comes home relaxed and gay may inflict less damage on her child than does the dour self-sacrificing worker-mother who returns to her home tense and tired, and who extends her daytime of physical absence by an evening of psychological absence behind a wall of glum self-absorption and resentment.

But what role do social forces play in determining *the level of awareness* on which the subsequent searching and defending maneuvers will operate? Where and how does repression enter into the dichotomy between the conscious-preconscious levels on the one hand and the unconscious processes on the other? And what role do social forces play in determining this primal step in the ontogeny of the neurotic process? As we have said, the search and defense may be painful on any level, but unless they are carried on under the dominant influence of unconscious forces they are not neurotogenic. Therefore the role of culture in neurotogenesis focuses sharply and precisely on the issue of how culture influences the psychological level on which conflicts are lived out. This would seem to be one of the basic questions which hitherto no one has either asked or answered.

3. Closely related to the central emotional positions and their unconscious maneuvers of search and/or defense are the many *trigger mechanisms* which precipitate human beings out of one emotional set and into another.

The relation of endogenous and social forces to the genesis of various kinds of trigger mechanisms and to their manifestations is another unexplored issue. Indeed we do not yet know whether social forces play any role in establishing those trigger stimuli which can explode almost any ingredient of human behavior; *e.g.,* the familiar phenomena of laughter, tears, terror and rage; or the explosive return to a central emotional position; or the release of obsessional and compulsive furors either of erotic and destructive impulses, or of symptomatic defenses against them; or the precipitation of sudden states of acute blocking and dissociation; or the induction of states of hypnotic dissociation; or of sleep and of its disruption. Clearly there are many triggers which can explode all manner of psychological processes. The roles of sociocultural influences must be assayed in relation to all of these.

Some readers may be perplexed at the introduction of the concept of trigger mechanisms at this point. If so, their perplexity is further evidence of the fact that the importance of these mechanisms has been overlooked both in normal psychology and in psychopathology. This is surprising in view of the fact that they are widespread in behavior. We are bombarded and beset by trigger phenomena through all of our waking hours, and during sleep as well. Yet consideration of trigger mechanisms has been limited chiefly to its most familiar manifestation, namely the classical phobia.

By trigger mechanisms we mean reactions which can be touched off by stimuli which are inherently neutral, but which have become highly charged not merely by conditioning, but through their preconscious and especially their unconscious overtones. There are many questions to ask about such phenomena; but the question which is relevant to the central preoccupation of this chapter is whether trigger mechanisms, either in their normal or in their pathological manifestations, are the products primarily of socio-cultural or of genetic forces, or of both. Or are they deviated pathologically by variants among social and cultural forces or by variants in genetic processes? If it should turn out that social and cultural forces play little or no role in their normal and pathological development, then we would be forced to conclude that here is one important area in the phenomenology of the neuroses in which such forces are of little consequence. This alone would be reason enough for presenting the issue as a challenge to a fresh exploration of the whole problem. Certainly for our understanding of the role of cultural and social forces in the genesis and the shaping of the neurosis, it is as important to know what areas of the neurotic process are relatively uninfluenced by them, as to know what areas are patterned by them.

I must say a few more words about this problem, although to develop the theme fully would require more space than available here. Its consideration falls into several categories: (1) the role of social and cultural forces in establishing trigger mechanisms at all; (2) their role in determining what are the effective triggers; (3) their role in determining thresholds; and (4) their role in influencing the form of the behavior which is released.

Before considering the first of these questions we would have to ask whether trigger mechanisms are inherent in the structural organization and biochemistry of the nervous system. Do they vary from one individual to another because of structural or biochemical differences? And if such differences exist, are they determined by innate and inherited genetic factors? And can experience in turn modify these hypothetical innate forces, or the trigger mechanisms which they establish? Do social and cultural forces influence the potential tendency of any nervous system to establish triggers, their thresholds, and/or the intensity of what is released by them?

There are many trigger stimuli; and we would expect social and cultural forces to play a role in determining which trigger stimuli are effective: animals, facial expressions, Yes or No signs, Stop and Go Ahead indicators, Hurry or Wait signals, wordless gestures or speech, silence or a loud noise, the seasons, changes in the weather, or the daily cycle of light and dark. The rustling of the wind through grass might well have a different trigger implication to a city-bred and a country-bred child. Indeed everything which has different implications in different cultures might carry different trigger connotations for different individuals. Yet this hardly seems to reach to the heart of the problem.

Finally one would have to ask whether cultural and social variants determine what is released, whether it will be panic or a free exhibition of rage or an inhibition of rage, or an inhibition of panic with its internalization, or tears or exultant laughter? Will social and cultural forces determine whether a certain trigger mechanism releases alertness or sleep, the paralysis and freezing of action or freedom to act? Or will social and cultural forces play a role in determining whether the discharge takes place largely through the autonomic nervous system?

Merely to compile a systematic and exhaustive list of the questions to be explored would enormously lengthen this article. At this point all that can be done is to emphasize both the basic importance of this issue and its total neglect.

4. Dependent in part on the early establishment of central emotional positions is the acquisition of perceptual processes and of motor organization and integration. This is a learning process which occurs during the ontogeny of the sub-verbal animal as well as in man. Every animal breeder can testify that the acquisition of these skills occurs in a field of emotional forces which influence it at every step. As this evolves in the human animal, however, one critical complication is added: to wit, the acquisition of the symbolic process, to which reference has already been made above and to which further consideration will now be given.

In the acquisition of perceptual, motor and symbolic skills, inherited or acquired differences in the organic structure of the central nervous system and of the perceptual and motor apparatus exercise an obvious influence. At the same time, the environment of every developing infant will also vary with respect to the amount of activity, handling and play, the amount of light and noise, the amount and quality of song, speech and gesture, of companionship and loneliness, of talkativeness or silence. His exposure to all of these will vary through his parents (and especially through his mother or her substitute), and through older siblings. Later the toddler's rivalry with older and/or younger siblings, his association with playmates who are more or less advanced, his conscious and unconscious fears of exposure and retaliation and therefore of victory, all influence his step-by-step acquisition of bodily and intellectual skills. At a later age the youngster who compensates in athletics for his limitations in the classroom, and his opposite number who does the reverse, are familiar to all of us. But always in the background lurk the figures of the parents, living out through their children their own fears, ambitions and vengeful retaliations for the unresolved aches and pains of their own childhoods.

All of these social and familial forces, and many others as well, influence the equipment which the child acquires, and with which he meets the demands of life. These are of great importance; because the freedom with which this learning process evolves will determine the richness and freedom with which the child will be able to utilize the vitally creative and essential economies of preconscious symbolic functions throughout the rest of his days.

5. This brings us to the next critical question: namely, at what stage in the evolution of the symbolic process are social influences recognizable as playing a role in determining the distribution between conscious and preconscious function on the one hand, and those distortions of the symbolic process which occur when any part of the symbolic process is split off from the rest and rendered unconscious by repression? (Whether comparable neurotic distortions occur through the influence of those ingredients in the unconscious system which have never been conscious and which consequently need never to have been subjected to the same kind of repression, is an issue which is relevant but not yet solved. Nor is it essential for our consideration of the central issue.)

With respect to our knowledge of every aspect of human development, it is fair to say that we are in a transitional period. We have learned enough to ask better questions. We have learned enough to know what we do not know. We have learned enough even to know a great many things that are wrong in human development. But we do not yet know how to improve them or even to alter them. For instance, we know that the human child develops with an enormous burden of masked and dissimulated hatred, rivalry and destructiveness. We know that to be sufficiently permissive towards the child as to give him the right to know what he feels makes a profound difference in the evolution of his processes of hate. Yet the child's destructive impulses remain intolerable to himself, causing him so much guilt and terror that he has to repress some measure of realization of them even in a tolerant environment. Thus cultured and familial tolerance makes a difference in the *point* at which repression occurs. But it does not eliminate repression. An eleven-year-old who was being helped to understand and to accept his blinding hatred of a younger sister suddenly turned suspicious eyes on his father and said with anger and with fear, "You mean I have to carry this hate around with me for the rest of my life?" Certainly it made a difference that the boy was able to ask the question. But it would be naive to assume that he did not still have to repress major aspects of his violent hatred.

Similarly, we may try to help children to accept the body, body products, and body apertures without the destroying and corroding fear and loathing of the body and all its works, which a blindly and crudely repressive educational system will engender. But in spite of efforts in this direction, we have not yet found out how to teach a child cleanliness and sphincter control and a reasonable degree of modesty without infecting him with body shame. We have not found out how to make that shift from the potty as a social event in the middle of the nursery floor to the toilet in the bathroom behind a closed door, without doing deep violence to the small child's self-acceptance. And in that act of violence, "modesty" is transmuted into shame, and repression arises.

Again we can make a difference in the social attitude toward sexual play and bodily curiosity. We can shift the point at which a barrier is interposed. It makes a difference whether a child has never seen an adult who is not fully clothed, or whether a child has seen an adult in underclothes, or whether a child has seen an adult naked. But the child who has seen the naked adult is not satisfied. He still wants to look inside. One child crouches in front of his mother and tries to look up through the screen of pubic hair, "to look up her little hole." Another says, "I wish you had a window in your belly so I could see what is inside." Curiosity is not satisfied with words or with inspection from a distance. The gratification of a child's curiosity requires touching and examining and smelling and tasting and exploring and looking inside. What a child sees he wants to get his hands on, and what he gets in his hands he wants to put in his mouth. Therefore tolerance does not rid us of barriers; but only changes their positioning.

This does indeed make a difference in the influence of repression. But it does not eliminate it. As a little boy was being bathed, he held onto his penis with one hand. Very gently his mother took the sponge, washed his face and his back and his chest and the free hand, and then with careful, casual nonchalance moved the hand that was engaged with his genital and sponged it too. Later the boy was restless and tense, and he told the story in these words, "And then a great big hand like a giant's hand came and tore my hand away." Subsequently, this evolved into a series of dreams of buzzard-like birds coming to pluck off his penis, because, as he said, the urine was their milk. Then he added reassuringly that his penis would grow back. Surely the careful attempt of the parent not to distort the child's attitudes toward his own genitals had made a difference. But the forces which distorted his perceptual processes of the mother's hand and produced their later symbolic representation, were by no means controlled by the parent's careful attitude. In the end, repression took over, to produce a dream which masked and altered the original experience, condensing with it the masked representation of recent experiences centering around watching his mother nursing his baby sister.

A woman of sixty looks back over the years to the age of three. She recalls the tin wash-tub in front of a pot-bellied stove on the sandy floor of a cabin on the western range where she grew up. She recalls the loving figure of her grandmother pouring hot water into the tub, and then sponging her down. Then comes the dark moment when in an altered voice the old woman thrusts the sponge into the little girl's hand and says, "And now you finish yourself off down there".... The words "down" and "down there" ring in her ears throughout the years like bells tolling a doom of fear and shame. "There is a dreadful nameless 'down-there' place on me to which even my loving grandmother will not apply a sponge or a name." Here was the birth of repression and of a "down-there" phobia that nearly blasted a brilliant life.

This is why I say that the forces of culture, whether deliberate and individual, or represented in the mores of a group, may battle against these deep repressive tendencies, but do not eliminate them. Nor do we know the full story of their interaction. Therefore we do not yet know the role of culture in this most critical moment in the evolution of the neurotic process.

6. Early in life, under influences which are so obscure that they have never been satisfactorily described, the phenomena of fixation enter the story of the development of the neurotic process. In the history of analytic theory this problem has been short-changed by looking upon the activity of erogenous zones and on the recognizable stages of instinctual development as constitutional or inherited determinants of the degrees of activity or of the demands of various organs as forms of instinctual gratification. These hypotheses beg vital questions: namely, what is fixation and what determines its advent on the scene, *i.e.*, what constellation of constitutional, physiological, individual and social forces combine to produce it? Later in life the manifestations of this same process constitute the familiar phenomenology of symptomatic compulsions, obsessions and phobias. It is at least possible that in early life similar compulsive mechanisms produce an obligatory and repetitive accentuation of some phase of instinctual activity, or focus on certain objects of instinctual need, and on the defenses which are set up against instinctual needs, whenever they become compulsively overdriven. If so, then fixations themselves are merely the early symptoms of a neurotic process which is already organized, and which is focused on instinctual functions. Later consequences of the original fixation should then be viewed as secondary derivatives of primary steps in neurotogenesis, with all of the neurotic distortions to which instinctual derivatives are vulnerable. This leaves the same plaguing questions still unanswered: (a) What role do social forces play in the formation of these primary fixations, *i.e.*, the early compulsive and phobic involvements of instinctual processes? and (b) what role do social forces play in the secondary and tertiary consequences of such primary fixations? (8)

7. Interwoven throughout each phase of this is the influence of those processes of identification which depend only indirectly on society or social structure or economic forces or cultural organization, but which are directly dependent upon the nature of the family organization, the size of the family unit, the number and variety of adults in whose aura the child develops, the presence or absence of aunts, uncles, and grandparents, the size, age, and sex distribution in the sibling group, the patriarchal or matriarchal domination of each family group, the monogamous-polygamous or uniandrous or polyandrous base of family formation, etc. Whether the family is happy or unhappy, whether it is a center of love

or of hostility and rivalry, these determine the identifications with adults which the child makes automatically, *i.e.*, preconsciously, and which in turn influence the evolution of the symbolic process, of the central emotional position, of compulsive distortions of instinctual processes, of compulsive and phobic defenses, and of the trigger mechanisms which can set any or all of these in motion.

These, then, are seven of the ingredients in the neurotic process. These ingredients are always at work. But we do not as yet have any precise methods by which to evaluate the influence of varying social, economic, cultural and educational influences on them. That there is an interplay is obvious. My argument is that in our effort to understand this interplay it is of great importance that we do not oversimplify it, and that we recognize the fact that as a first step in this direction we must become clearer about the impact of those universal individual experiences which occur in every culture of which we have any knowledge, and which initiate that most unique of all developmental phenomena in the human: namely, the distortion of symbolic function which we call the neurotic process.

A penetrating, incisive, clear and economical summary of the contribution of the allied disciplines to our understanding of psychopathology is to be found in Sidney Axelrad's paper in the 1955 Symposium on Progress in Orthopsychiatry (1), together with a carefully selected bibliography. His concluding paragraph is in many ways the essence of what I have been trying to say here.

"It seems to me that part of the confusion in the relation of the social sciences and orthopsychiatry is caused by an assumption that the social sciences stand to psychology as biochemistry and anatomy and physiology stand to medicine, or to the normal functioning of the individual. I am afraid that the situation is reversed. It is psychology, the study of what goes on inside the individual, and very largely of what goes on at a level not accessible to consciousness, that is the basic science for the social sciences. The clearer we are about this, the richer will be the cooperation between the social sciences and orthopsychiatry."

SUMMARY

We must, in short, consider the interaction of developmental universals and of cultural variables on those ingredients of the neurotic process which are unique for humankind: to wit, the establishing of central emotional positions, the unconscious efforts to preserve certain of these and to defend against others, the acquisition of perceptual and motor patterns, the dichotomy or trichotomy of the evolving symbolic process, the subordination of instinctual development and of the defenses against its various ingredients to unconscious symbolic functions, the fixation of instinctual development by superimposed obsessional, compulsive and phobic processes and also by dreamlike infantile delusions, the development of manifold disruptive trigger mechanisms, and the integrative and dispersive influences which arise through distortions of the normal processes of identification.

The error which has been made in most considerations of the interaction between developmental and cultural forces in the neurotic processes is identical with the fallacy which crops up repeatedly in work on the so-called experimental neurosis. This has been to confuse the seed or the soil with the plant, the part with the whole. Significant external stresses and the emotional disturbances which arise in certain phases of the neurotic process (but which are not the essential distinguishing mark of the human neurosis) have been confused with the unique neurotic process itself. It is this double fallacy which we have tried to avoid in this paper.

REFERENCES

1. Axelrad, S. Symposium on progress in orthopsychiatry. Am. J. Orthopsychiat., **25**: 524–537, 1955.
2. Hollingshead, A. B. and Redlich, F. C. Social stratification and psychiatric disorders. Am. Social Rev., **18**: 163–169, 1953.
3. Hollingshead, A. B. and Freedman, L. Z. Social class and the treatment of neurotics. The Social Welfare Forum, 1955, pp. 194–205.
4. Kubie, L. S. The neurotic potential, the neurotic process, and the neurotic state. U. S. Armed Forces M. J., **2**: 1–12, 1951.
5. Kubie, L. S. Husband and wife. In *The People in Your Life*, Hughes, M. M., Ed., Chap. 2. A. A. Knopf, New York, 1951.
6. Kubie, L. S. The fundamental nature of the distinction between normality and neurosis. Psychoanalyt. Quart., **23**: 167–204, 1954.
7. Kubie, L. S. Say you're sorry. Psychoanalytic Study of the Child, **10**: 289–299, 1955.
8. Kubie, L. S. The influence of symbolic processes on the role of instincts in human behavior. Psychosom. Med., **18**: 189–208, 1956.
9. Leavy, S. A. and Freedman, L. Z. Psychoneurosis and economic life. Unpublished manuscript.
10. Spitz, R. A. Hospitalism. Psychoanalytic Study of the Child, **1**: 53–75, 1945; **2**: 113–119, 1946.

BOOK REVIEWS

Kubie, Lawrence S. *Neurotic Distortion of the Creative Process.* University of Kansas Press, Lawrence, Kansas, 1958. v + 151 pp. $3.00

There is much in this book to excite and justify admiration. It is full of interesting and sometimes original ideas; it is informed by a wide and intimate knowledge of analytic theory and practice; it shows that its author is conversant with and sensitive to a number of the creative arts; it is written for the most part in a clear and direct style without undue laboring of points or unnecessary syntactical complications; and finally it is ennobled by a high moral purpose to understand and improve the human condition. This surely adds up to an important achievement. Still, after carefully reading the book, this reviewer somehow feels rather let down and disappointed. Knowing Dr. Kubie, perhaps I unconsciously expected him to perform a miracle.

I say a "miracle," for at least some of the problems he tackles in this book are impossible to solve, by rigorously logical or scientific or even more "creative" methods, in the present state of our confusion and ignorance regarding many aspects of these problems. In some instances, they cannot even be clearly defined, so that one knows precisely what question is being asked and hence what observations would answer it. But before making any further critical judgments, let me first state what I take to be the central thesis of the book.

As indicated in earlier papers, Kubie distinguishes between conscious, preconscious, and unconscious systems of symbolic processes. But, at the same time, he emphasizes that every act is the product of the concurrent operation of symbolic processes in all these "systems." Furthermore, he looks upon the conscious and unconscious "poles" of this tripartite set of psychic functions as representing two sorts of threats or blocks to our "creativity" and, more widely, to our good adjustment and mental health. To try to explain this briefly, he seems to believe that to the degree that a given act is under the influence of unconscious processes it is "anchored in unreality," which implies that, in so far as this is true, we are the victims of a no longer existent past that is yet so built into us that its misrepresentations "distort" the symbolic processes of our minds and block our efforts to understand our present self, to adjust to current realities, or to exercise fully our creative capacities. On the other hand, if the act is dominated by conscious processes, it is said to be "anchored in reality"—which some might consider good, being a condition of sanity and reality-testing operations, but Kubie sees it as at least partly or potentially bad. For as conventionally represented in words, as rigidly sanctioned by our unquestioningly accepted mores, and as irrationally reinforced by some current educational procedures, these outer "realities" may become blinders on free intelligence, lead to slavish conformity, aggravate neurotic tendencies, and so on. Now while both kinds of "anchoring" exist, and on the facts available seem to be inevitable, Kubie's hopeful thesis is that we may learn to reduce and control the harmful influences of both these "poles." By so doing, we may free our minds, on the one hand, from the blind and oppressive, stereotyped and repetitive, unconscious processes, which variously block, distort, and defeat our efforts to be either flexibly adaptive or valuably creative, and —on the other hand—we may prevent the largely stultifying and rigidifying and sometimes quite paralyzing effects of much modern, but not intelligently "progressive" education. As a result, our "naturally" creative and flexible preconscious processes, whence cometh original ideas in both science and art, will be freed to make their distinctively human contribution and will, accordingly, enormously enrich human life. This suggests a "free" society in which psychoanalysts would have to be at once parents, educators, and kings.

But no abstract summary can possibly do justice to the complex insights or the cumulative force of Kubie's argument. Nor is it possible in a brief review to examine logically his various assumptions, which in such a subject are perhaps unavoidably obscure and hard to test by empirical operations; or to evaluate the clarity and utility of his key definitions, which too often are left quite inexplicit, and hence must be doubtfully inferred from different contexts of use; or to analyze and discuss the many examples drawn from the arts, the sciences, from his clinical experience and from everyday life, which Kubie uses to illustrate and support his thesis. With respect to his clinical examples, while interesting and suggestive in themselves, he seems rarely able to use them successfully in order to prove any particular statement that might be inferred from his broader metapsychological hypotheses. No doubt this is due, at least in part, to the need to preserve the incognito of his patients. But it is also due, I fear, to the appalling complexity and obscurity of the symbolic processes which Kubie talks about and tries to explain. The result, perhaps inevitably, is that part of the time the reader does not know for sure which questions have been answered on the basis of relevant facts and which have been begged by the use of definitions which, though vaguely persuasive, are not adequately clarified or defended.

Kubie will probably disagree, but my personal judgment is that a good deal more logical clarity could be brought into his argument if he were not so afraid of definitional "strait jackets," and if he focused his obviously more than sufficient intellectual powers on trying to say quite explicitly and as unambiguously as possible exactly what he means by such key terms as "symbolic process," "neurotic distortion," "obligatory act," "creative process," "rigid behavior," and the like. In addition, It think that what is needed is not only to "say" what these meanings are in connotative definitions, but the definitions must be illustrated by and checked against various concrete examples—works of art, case histories, and so on—which are *critically and thoroughly analyzed* in a determined effort to eliminate the ambiguities and to reduce the obscurities which inevitably infest all general definitions. Psychoanalysis, more than most subjects, requires a constant "running back and forth" (symbolically and sometimes overtly) between the universal, *qua* thought, and the particular, *qua* sensed, in order, as Kant suggested, to keep the former from being "empty" and the latter "blind." But our attempts to bring concepts and percepts, theory and practice, into fruitful relations, need to be carried out as rigorously and systematically as possible. Otherwise, the results—however interesting as clinical anecdotes or literary psychology—will not even approach being scientifically organized and tested knowledge.

To make my criticism a little more specific, I feel that the argument as to which kind of symbolic process is predominantly expressed in, or which constellation of forces mainly produces, a given "act," whether (say) an inner thought-process or an overt behavior-sequence, is often circular. By definition, the "Unconscious" is a lot of "bad" things: it is said to be rigid, stereotyped, unmodifiable by learning and hence often inflexibly maladapted to new situations. By contrast, the "Preconscious" is said to be free, creative, flexible, and so on—in short, a number of "good" things. Hence, when some behavior is "observed" (this includes of course many inferences) to be rigid, stereotyped, and compulsively repeated in inappropriate situations, we may argue that such behavior is preponderantly caused by unconscious processes to which we have already assigned the very same properties. The logical difficulty here cannot be avoided (not that Kubie does this) by saying, "Oh well, of course there *is* no thing, region, or set of psychic processes, the unconscious: 'unconscious' is just a *word* that designates a metapsychological construct." For we cannot say that the unconscious is a "construct" in the mind of some theorist and then proceed to talk about "forces" in *it* "distorting" symbolic processes or "blocking" creative efforts in the mind of some patient. To do this, is to fall a victim of symbol-referent ambiguity and to assign to ideas, *after dislocation from their proper context,* the causal properties of their contingent referents. If we are discussing a vague and complicated subject on a high level of generality, this fallacy both expresses and aggravates a very basic kind of confusion, which it takes an expert in logic who is very familiar with the subject-matter, even to diagnose correctly. The notion, which is rather widespread, that such projected and circularly "validated" constructs help us to "understand," let alone predict and control, behavior sequences, is one of the more sophisticated delusions of our time.

Let us examine a little more carefully Kubie's theory of the relations between Cs, Pcs, and Ucs processes. Regarding these systems of processes, he states, "At every moment of life, all three are active concurrently. As a consequence, whether we are sick or well everything we ever do or say or think or feel is a composite resultant of them all." But some pages later Kubie speaks of the "incessant bombardment of the human psychological apparatus by preconscious influences which give shape to our thoughts, feelings, and purposes, independently of conscious or unconscious factors." I do not see how to reconcile logically these statements. If "*all* three are active concurrently," and if "*everything* we ever do or say or think or feel is a *composite resultant* of them *all*," then how can "preconscious influences" ever operate "*independently* of conscious or unconscious factors" (italics mine)? Has the Law of Non-Contradiction been repealed?

Now, in this connection, let us consider one of Kubie's examples. "In tracing a path between the laboratory and her home a woman scientist automatically found herself choosing a certain sequence of streets and turns rather than its mirror image. The two paths were precisely the same distance. There was nothing to choose between them with respect to the pleasantness or unpleasantness of the walk. But she had a strong preference for one over the other. As she allowed her thoughts to roam more freely than her steps, it became clear to her that one route traced a letter which was the first letter in the name of the person who was the major source of pain in her life, a fact which she did not want to face openly. She did not want to acknowledge that she knew that her husband was a homosexual, and she did not want to trace the letter with which the name of his boy friend began. As her feet traversed the streets on her daily path, an automatic Preconscious, but not Unconscious process, chose her path for her."

How are we to interpret Kubie's final statement, "an automatic Preconscious, but not Unconscious process, chose her path for her"? Setting aside conscious choice, is choice (in Kubie's theory) the kind of mental act which, *by definition*, is restricted to the preconscious and excluded from the unconscious system? If so, the "conclusion" stated above cannot be self-consistently denied by Kubie or anyone else who accepts his definitions. Now definitions, being optional *rules* of language, are not logically the sorts of things that can be disproved, for they do not make empirically testable truth-claims. Hence if Kubie intends the logical function of the term "choice" in his concluding statement to be so interpreted, no one can prove that the conclusion is false; for on such assumptions the conclusion becomes "necessarily" true, *i.e.*, it could not, regardless of the contingent facts, be false. This part of his system would, on this definitional assumption, be like a map which is adhered to and "symbolically" followed even though there is no existent territory that corresponds to it—a situation that would make the term "map" misleading, to say the least. So I assume Kubie would prefer to defend his statement by some less "formal" method, in which the locus of control—what sanctions the statement—finally resides in the empirical facts of the case. If so, then the question is left "open" and can only be closed by some combination of accepted observations and warranted inferences.

But now the clinical plot thickens, for even the facts in the brief "history" given suggest more than the possibility that a number of "Unconscious" factors also operated to determine her "Preconscious" choice. But quite aside from the debatable truth of any particular "deeper" interpretation, here again I find Kubie's concluding statement inconsistent with the other general statements above quoted, for example, that every act is the "composite resultant" of forces in "all" the systems of the mind. If he had written in effect, "while an automatic Preconscious chose her path for her, this choice was most probably—indeed, in accordance with my system, necessarily—influenced or determined to some degree by both conscious and Unconscious processes," he would have been consistent and (my guess is) would also have stated correctly the facts of the case.

It *may* appear to some readers (including possibly Dr. Kubie!) that I am making a lot of a small point. But I hope not, for my conviction is that this kind of point appears "small" only to those who do not understand its implications, which are far-reaching and quite fundamental. For to know what statements are consistent with our premises, and what guarantees the validity of our conclusions, and whether these conclusions are in principle subject to disproof if the contingent facts of nature turn out to be different from what we claim, to know these things is to know *what* we are asserting, and *whether* or not this assertion is warranted, and if so on what *kind* of grounds. Surely *these* are not trivial matters—unless we are going to abdicate reason altogether.

Now while I greatly admire a number of things in Kubie's book, I do find it very difficult to evaluate some of his central theses and recurrent interpretations in accordance with the logical norms of meaning, validity, and truth. To be sure, the subject matter is inherently obscure and there are few agreed-upon procedures for asking the "right" questions, let alone answering them, and key definitions, in many contexts, are still matters of dispute. These are extenuating circumstances. Still, Kubie could have more squarely and consistently faced up to basic logical issues in this book. Instead, he tends too often to glide gracefully over or around them, and to rely too much on his preconscious inspiration and engaging style to "support" the onward sweep of his discourse as well as some of his conclusions. This makes for highly readable prose, but hardly scientific proof.

I do not, of course, wish to imply that there are no clinical data or experimental facts which give empirical support to the systematic distinctions made by Kubie. For as every informed reader knows, a vast mass of psychoanalytic data, some of them rather highly interpreted, suggest *some* such distinctions and a lot of experimental work relating to hypnotic induction and its effects, to tachistoscopic observations, and so on, also suggests the utility—some would say the necessity—of somehow taking into account differences in symbolic processes with respect to their accessibility to conscious inspection, their relation to learning and unlearning, to unconscious needs and drives, and so on. But these facts are not understood well enough to justify fully the systematic distinctions and interpretations made by Kubie, which in some cases are too vague to be clearly grasped and in others are too "precise" (I am thinking of some of his diagrams) to be rationally believed. We must fall back, I suppose, on the usual scientific faith that what we need in this field are clearer ideas, better methods, and more well understood facts.

With respect to metapsychology, what we particularly need is a set of *good* constructs. By this I mean that the terms used to designate the form and properties of the construct must be clearly and unambiguously defined. For without this we cannot see the implications of the construct, nor can we infer testable statements from it, *i.e.*, statements which can be demonstrated to be mutually consistent or to be mediated by valid inferences or to be in accord with perceived facts. But in addition to fulfilling these necessary conditions, the construct ought, if it is to be scientifically fruitful, to represent a useful selection (which will vary with our purposes) from among the infinite possibilities open to the inquiring mind. This criterion probably means that the empirical referents, which may be coordinated with the symbolic construct, must be related, in causally important ways, with events in the "executive order of nature." It is in fulfilling this latter condition that highly creative, but also veridically perceptive, scientific genius is often needed. If, finally, these logically and empirically supported inferences can be quantified, or the referents of their terms can be measured, we would have still better or more scientific constructs—at least according to one rather widely accepted definition of "scientific."

It is not to Kubie's discredit that he has failed to furnish us with such a set of metapsychological constructs. No one else has either. But a critical reader would be reassured sooner, regarding the author's own logical standards, if Kubie had not waited until two thirds through his book to admit "that we still lack instruments by which to estimate, even approximately, the relative roles of the three conceptual and symbolic systems which participate concurrently in all such phenomena, *i.e.*, in the imaginative creations of health and in the equally imaginative creations of illness." For this admission, while admirable in its candor, is grave in its implications, since the central thesis of his book depends logically on being able to make and validate just such "estimates."

Wittgenstein once ironically remarked, that what cannot be said cannot be said, nor can it be whistled. However, in spite of contrary winds of doctrine, or the (sometimes) discouraging insights of logic, everyone interested in understanding human behavior and its disorders, or the problems of artistic and scientific creativity, ought to read Kubie's book. If he has partly "failed," I am sure that he is aware of it; and if this is consolation, so would anyone else. As for his many successes, in illuminating—often with grace and brilliance—this or that phase of the problems involved, every reader will be grateful.

John R. Reid

Scientific and Technical Translating and Other Aspects of the Language Problem. A Report Published by UNESCO, Paris, and circulated through the Columbia University Press, New York, 1957. pp. 282, $4.00.

This document is of the first importance to everyone concerned with international communication; especially so to scientific investigators and to closely allied professional people. The report arises from the final part of a resolution adopted at the seventh and eighth sessions of the General Conference of UNESCO in 1952 and 1954, which authorized the Director-General "to advise and encourage international organizations concerned with the development and improvement of scientific documentation, with the standardization of terminology, with the compilation of multilingual dictionaries, and with the improvement of scientific translations." Many people contributed to this vital effort which is presented at this time as a statement of progress put forward in the hope that it may stimulate proposals for positive action. It sets forth the existing status in the world of the myriad problems of language, translation, possible internationally understood special languages, terminology and lexicography. The problems are so many and so basic that they cannot be gone into here. Nor is this intended to be a review in the usual sense. The report cannot be reviewed as such but is of enough importance to warrant bringing it to the attention of as many people as possible. It should be read carefully and thought about at length. Its value in relation to the future development of our culture cannot be questioned.

Following is a list of the proposals discussed regarding which further expressions of opinion are desired:

1. Encouragement to the formation in other countries of registers of specialist translators similar to that which exists in the United Kingdom, and to the linking of these internationally.

2. Promotion of a journal or journals of translated articles.

3. Study of the practicability and possible advantages of publishing journals in which the "essential" passages, translated into several languages and printed in parallel columns, would alternate with "unessential" or supporting passages printed across the page in the original language only, along with mathematics and illustrations.

4. Appraisal of the economic and other aspects of the various possibilities that exist for pooling, indexing and making copies available of unpublished translations, both locally and internationally.

5. Promotion, on the international plane, of a declaration by copyright owners which would allow translations to be made under conditions similar to those under which copying is allowed by signatories to the Fair Copying Declaration promoted by the Royal Society of London.

6. Encouragement of the formation in different cities of societies on the lines of the former Engineers' German Circle in London, to afford practice in discussing technical subjects in foreign languages.

7. Preparation and publication of monographs treating the fundamentals of scientific subjects in various languages for the benefit of foreigners desiring to improve their reading ability.

8. Institution of courses to help in developing those abilities which are useful for "technical translation editors."

9. Co-operation between UNESCO and suitable national bodies in attesting the possession of suitable experience and qualifications to undertake translating in specialized technical fields.

10. Calling of a conference of representatives from countries using minor languages to afford them an opportunity of agreeing which major language is preferable as a medium for giving their own scientific publications international currency.

11. Further study of the expediency of promoting the use of an international auxiliary language for scientific publication or abstracting or both.

12. Recommendation of a principle for deciding the conditions under which internationally current special terms may be preferable to terms derived from native roots in each language.

13. Assistance in the establishment of "terminological bureaux" both internationally for particular subject fields and locally for particular languages.

Robert G. Grenell

Wolstenholme, G. E. W. and O'Connor, Cecelia M., Eds. (For the Ciba Foundation). *The Cerebrospinal Fluid.* Little, Brown & Co., Boston, 1958. xii + 335 pp., illus. $9.00

As one of its valuable series of symposia the Ciba Foundation here presents a discussion of the questions of production, circulation and absorption of the cerebrospinal fluid. Anatomists, neurologists, pathologists, psychiatrists, pharmacologists and neurosurgeons gathered to discuss anatomy of the choroid plexuses, arachnoid villi and related structures; to ask whether CSF appears to be a secretion, excretion or transudate; to explain blood-brain and blood-CSF barriers; and among many other problems to attempt to correlate fundamental considerations with clinico-pathologic observation.

Professor Kappers surveys the development of the choroid plexus and there is a beautiful presentation by Drs. Wislocki and Ladman of the results of study of the plexus with the electron microscope. Their findings certainly suggest that very active processes are going on. These structures are of particular interest in relation to the broad problems of membrane structure and permeability in the central nervous system especially in the light of their description of capillary pores in the plexus. Many things are touched on, such as the necessity for further histochemical studies of the plexuses as well as detailed examination of their general structure, topography and nerve supply.

Confusion still reigns concerning the circulation and absorption of CSF; confusion of a degree only exceeded by that relative to the "barriers." The discussion shows that there are several meanings of the word barrier and that at the moment many barriers exist at both the morphological and physiological levels. It will probably be surprising to many that so little definite progress has been made in this area despite the modern technical advances. This serves to show another sphere in which work on the central nervous system is unusually difficult, complex and frustrating.

It is obvious that the symposium will be of undoubted value to all those interested in nervous system problems.

Robert G. Grenell